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FARTHER
INQUIRIES
INTO THE CHANGES INDUCED ON
ATMOSPHERIC AIR,
&c.

ATMOSPHERIC AIR,

INTO THE CHAMBER INDICED ON

INQUIRIES

FARTHER

AND ON THE

INQUIRIES

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INTO
THE CHANGES INDUCED
ON
ATMOSPHERIC AIR,
BY
THE GERMINATION OF SEEDS,
THE VEGETATION OF PLANTS, AND
THE RESPIRATION OF ANIMALS.

BY
DANIEL ELLIS.

EDINBURGH:
PRINTED FOR W. BLACKWOOD, SOUTH BRIDGE STREET;
AND J. MURRAY, 32. FLEET STREET,
LONDON.

1811.



THE CHANGES INDUCED

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ADVERTISEMENT.

AT the close of his former publication, the Author intimated his intention of prosecuting his inquiries, if the public approbation of his labours should be such as to encourage their continuance, and future experience should appear to confirm his views. An interval of four years has now elapsed since the date of his former Work, during which period many important additions have been made to the various branches of research connected with the subjects discussed in this volume; and the new views, which the late discoveries of Mr Davy have introduced into Chemical Science, may also be expected to extend their influence to all its Physiological applications. From these causes, joined to the Author's endeavours to correct some of his former opinions, and to illustrate and improve others, the additions which he now brings forward, have insensibly acquired a size greater than that of his original Work.

In these additional Inquiries, the Author has greatly extended his researches into the “Changes induced on the Air by the Vegetation of Plants;” and if his success bear any proportion to the pains which he has taken, he hopes that he may be found to have disentangled some of the perplexities, and reconciled some of the differences which have hitherto prevailed on this important subject. The views which he has suggested respecting the “Colours of Plants” arose naturally in the order in which they are presented; and the attempt to connect them with other curious operations in the vegetable economy, will, he trusts, add considerably to their interest and value. If an apology be deemed necessary for the length to which he has extended the section on the “Agency of Light,” he would beg to state, that some parts of that section were composed with reference to ulterior views, which, for the present, he has been induced to postpone.

On the second branch of his Inquiries, namely, that which relates to “Animal Respiration,” the Author has diligently sought information from all the sources which lay open to him; and the important additions, which the labours of his Contemporaries have enabled him to make to this part of his Work, will, he trusts, be considered not only as valuable in themselves, but as tending to augment the confidence of his readers in the conclusions to which he had before endeavoured to conduct them. To the chapter on the “Source

of Carbon," he has also been enabled to add many new facts; and the illustrations which he has now subjoined will, he hopes, remove some objections to his doctrine, and more completely develope and recommend his views.

In contending with so many opposing facts, and such variety of conflicting opinions, the Author can scarcely presume to hope that he has always escaped error; but, he trusts, he may claim the praise of having diligently sought the truth. While too, on all occasions, he has exercised the most perfect freedom of discussion, he hopes that he has uniformly expressed himself in the language of one who would wish always to combine a just respect for the opinions of others, with the attachment he may feel to his own. Throughout his Work, it has been his anxious wish "to render unto Cæsar the things that are Cæsar's;" and if, through ignorance or inadvertence, any fact or opinion should not be assigned to its rightful owner, he entreats the reader to correct his mistake, but not hastily to pronounce it wilful; for he can truly declare, that, though not insensible to the desire of an honest reputation, he would not knowingly invade the mental rights and property of another for all the fame which all the discoveries in all the sciences could ever give.

EDINBURGH, }
MARCH 20. 1811. }

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London,
March 30. 1813.

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ERRATA.

- Page 52 Note, for *Physiologia* read *Phytologia*
 — 72 line 26 for *vinea* read *vinca*
 — 155 Note, for tom. iii. read tom. iv.
 — 340 — 25 for *bottles* read *bottle*
 — *ibid* — 26 for *they were* read *it was*

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Primo enim paranda est *Historia Naturalis*, et *Experimentalis*, sufficiens et bona; quod fundamentum rei est: Neque enim fingendum, aut excogitandum, sed invenendum, quid Natura faciat, aut ferat.

BACON.

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ERRATA.

Page 32. Note. For Physiology read Enyologia
 — 72 line 26 For vixit read vixit
 — 122 line 10 For corn. ill. read corn. iv.
 — 210 line 25 For hostis read hostis
 — 210 line 26 For hostis read hostis

ADDITIONS TO CHAP. I.

OF THE CHANGES INDUCED ON THE AIR BY THE GERMINATION OF SEEDS.

* 200. **I**N our former publication, we distinctly considered the operation of water, heat, and air, in promoting the germination of seeds; and the conclusions to which we then came are, for the most part, acquiesced in by chemists. The facts, indeed, on which they rest, are so numerous and precise, as scarcely to leave room for any difference of opinion respecting them. Such additional evidence, however, as we have since been able to collect in their support, we shall now briefly notice, preserving, as nearly as we are able, the arrangement observed in our former treatise.

201. The presence of water has been stated (1.) as one of the first agents required for the commencement of the germinating process; and the force which seeds, by imbibing it, are capable of exerting, is well illustrated in some experiments made by Mr

* This and the following numbers are continuations of those which mark the paragraphs in the former publication.

Boyle. He filled several strong phials with horse beans, and then pouring water upon them, he corked the phials, and tied them down. As the seeds imbibed the water and increased in bulk, many of the phials were burst asunder. To make a nearer estimate of the expansive force of the swelling beans, he procured hollow cylinders of brass, of different lengths and diameters, to which wooden plugs were fitted, in such a manner as to move freely up and down in the cylinders as the seeds should swell. A cylinder of this kind, six inches in length and two in diameter, was filled with beans, which were then covered over with water; and as the seeds imbibed the water, they raised the plug, although a common half-hundred weight was laid upon it. When the cylinder was less than six inches deep, and was four inches broad, the beans, in swelling, would raise the plug, although loaded with a weight of above one hundred pounds*.

202. We before remarked, that this swelling of seeds took place equally in water that contained air, and in that which had been previously deprived of its air by long boiling. Our observations on this point are confirmed by the experiments of M. F. de Saussure. He found that seeds of coffee, which had even lost the faculty of germinating, exhibited, nevertheless, a partial development of their radicle, not only in common, but in boiled water, and even in vinegar and saline solutions. This enlargement, however, is not to be considered as a true germina-

* Boyle's Works, by Shaw, vol. ii. p. 285. 2d edit.

tion; for it neither changes the taste, the odour, nor any other properties of the seed *.

203. To that swelling of the seed which constitutes germination, M. Scheele considered the presence of oxygen gas in the water to be necessary, and asserted that this gas was converted into carbonic acid†. According, also, to M. de Saussure, peas and other seeds germinate in water that contains oxygen, but do not properly unfold their radicles in boiled water, when the water does not exceed more than seven or eight times the weight of the seed; but when it is one or 200 times as great, the radicles have then been prolonged. This happens, says he, because ebullition does not entirely deprive water of its air, and when, therefore, the quantity of this fluid is sufficiently large, enough of air is present to excite a feeble development of the seed ‡.

204. Water enters into the seed, partly through the pores of its external coat, and partly through the small opening placed in the *hilum* or scar. M. Senebier asserts, that if the opening in this scar be closed, the swelling or germination of the seed is prevented§; but in some experiments made by Dr Jones, this was not found to happen. He placed some large beans in a saucer of mould with the opening in the scar uppermost, and watered them in such a manner that the fluid could not enter it, and yet they seemed to germinate in the ordinary time.

* Recherches Chim. sur la Vegetation, p. 2.

† On Air and Fire, p. 167. ‡ Recherches, p. 3.

§ Mem. Physico-Chimiques, tom. iii. p. 333.

205. The entrance of water, in the first instance, seems to be the effect of simple imbibition ; but after penetrating the external coat, the fluid appears to be conveyed by a vascular structure. M. Senebier found, that if seeds, after being immersed in coloured liquors, were cut into thin slices, they exhibited coloured points through the substance of the cotyledons, which indicated a vascular structure. The colour became less and less as it approached the germ ; and from the plume of a bean, when cut through, a transparent fluid escaped from five vessels, arranged in a circular order. The two vessels which united the cotyledons to the plantule were full of juice, which, on being held up to the light, exhibited a slight tinge of colour *.

206. The agency of heat in promoting the development of the seed is universally admitted ; but the operation of light has been much disputed. From a review of the facts, we were before led to conclude (3.) that light, simply, possessed little power either in promoting or retarding germination. M. Michelotti, however, supports the opinion of M. Senebier, that light is injurious to this process † : but the experiments of De Saussure seem to prove that this arises only from the operation of the heat that exists in the solar beam, when seeds are confined in close vessels ; for when the same temperature was preserved, they germinated equally well in transparent and opaque vessels ‡.

* *Physiol. Veg.* v. iii. p. 362. et seq.

† *Philosophical Magazine*, v. ix. p. 246.

‡ *Recherches*, p. 23.

207. The first correct experiments, made to ascertain the specific changes induced on atmospheric air by the growth of seeds, were published by the celebrated Scheele, in his treatise on Air and Fire, about the year 1777. He found that the air, in which moistened peas were made to grow, underwent no change of volume; but that its oxygen gas disappeared, and a quantity of carbonic acid was produced. As the quantity of acid formed equalled in bulk that of the oxygen gas which the air naturally contained, he proceeded to ascertain whether this gas had not been converted into the acid in question. With this view, he mixed together, in a bottle, one part of oxygen with three parts of nitrogen gas, and put some peas into it, with water sufficient to carry on their germination. The peas soon began to grow, and when they grew no more, he observed no variation in the bulk of air, but one-fourth of this air was attracted by milk of lime. It is therefore, says he, the oxygen gas of the air which, by germination, is changed into carbonic acid. This conclusion he farther established by confining peas in pure oxygen gas, where, though they did not grow so well, yet they changed it into carbonic acid; and had he continued the experiment a sufficient time, he had no doubt but that the whole of this gas would have experienced a similar change*.

208. Dr Ingenhousz confirmed and extended these facts, by proving that every kind of air, which was fatal to animal life, was incapable of supporting ger-

* On Air and Fire, p. 151. 156.

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* On Air and Fire, p. 151. 156.

mination, either in the sunshine or in the shade; but that seeds grew both in common air and in oxygen gas, and formed carbonic acid in each *. The subsequent experiments of Gough, Cruickshank, and De Saussure; and those which have been already related (9. et seq.), confirm, in all respects, the conclusions of Scheele. In his late Researches, also, De Saussure extended his experiments to a great variety of seeds, and, in all that he tried, he found the bulk of carbonic acid produced to be exactly equal to that of the oxygen gas which had disappeared †. These various experiments seem to establish, beyond the reach of doubt, the facts, that oxygen gas is essential to germination; that it is converted into carbonic acid in that process; and that the bulk of acid gas produced is exactly equal to that of the oxygen gas which disappears.

209. In the former part of our inquiry (12.) we stated the bulk of carbonic acid, formed in germination, to be somewhat less than that of the oxygen consumed, which we ascribed to the *condensation*, supposed, at that time, to attend the conversion of oxygen gas into carbonic acid. We then, however, mentioned many difficulties which opposed the accurate determination of this point; and our opinion at the time, was at variance with the observations of Scheele, of Cruickshank, and of De Saussure. We have since found, in many trials, that no apparent diminution of the volume of air occurs in germina-

* Experiences sur les Vegetaux, tom. ii. p. 2. 10.

† Recherches Chim. p. 9. 10.

tion : but in other instances, this has, in a small degree, appeared to happen. Aware, however, of the various causes by which the volume of air may, to a small extent, be affected, and calling to mind the late accurate experiments of Messrs Allen and Pepys*, in which no diminution of volume was found to attend the conversion of oxygen gas into carbonic acid by combustion, as Crawford and others had previously supposed, we do not hesitate to express our conviction, that the bulk of carbonic acid produced in germination is, like that in combustion, exactly equal to that of the oxygen gas which has disappeared.

210. In thus stating the volume of carbonic acid, produced by seeds, to be equal to that of oxygen consumed, it is necessary to bear in mind that we speak of its formation in the process of germination alone. We have before shewn, that moistened seeds equally produce carbonic acid when confined in nitrogen (5.), or in tubes filled with mercury (19.); but no sign of development is then observed, and the volume of gas, instead of continuing the same, is greatly augmented. These experiments have been repeated by Mr Acton, who found the formation of carbonic acid by seeds, introduced into a tube of mercury, to commence in a few minutes; and in twenty-four hours the air, on analysis, afforded $\frac{20}{100}$ of carbonic acid, although no evident sign of putrefaction appeared. Hence he contends, that, since seeds thus form carbonic acid when no oxygen gas is present, it must be difficult, if not impossible, to fix

* Phil. Transact. 1807.

the precise period at which germination ceases in oxygen gas ; or to determine what part of the acid is really formed by the process of germination, and what by the spontaneous decomposition of the seed. Consequently, the attempts to establish an identity of quantity, between the oxygen lost and the acid produced, must be fallacious, and the conclusion thence derived unworthy of credit *.

211. We readily admit this difficulty in estimating the quantity of carbonic acid ; but a due attention to circumstances will, we think, enable us entirely to remove it. To this end, it is important to observe, that, where this acid is formed without the presence of oxygen gas, no apparent change is effected in the seed, but the bulk of air is increased ; where, on the contrary, it is produced in that gas, the oxygen disappears, and the evolution of the seed goes on, but the volume of air suffers little or no variation. Whether, therefore, we attend to the sensible effects produced in the air, or to those which occur in the seed, the difference is most striking ; and so likewise must be the causes from which such difference proceeds. These causes, we have argued (19.), arise from the seed, in one case, affording only carbon, which unites with the oxygen gas of the air, and converts it into carbonic acid ; while, in the other case, this acid gas is at once furnished by the seed alone. The one result, therefore, accompanies the evolution of the seed, the other attends its spontaneous decomposition ; but it is only with the former process that we are at present concerned.

* Nicholson's Journal, vol. xxiii. p. 217 et seq.

212. Now, in the experiments on germination already related (8. 9.), the gradual disappearance of oxygen and production of carbonic acid seem sufficiently to establish their conversion into each other ; and the stationary bulk which the residual air maintained, after all the oxygen was consumed, together with the equality of volume between the above-named gases, appear, likewise, to prove that none of this acid gas was furnished by the seed alone. If the seed, in its germination, had furnished both the elements of carbonic acid, then, as in its decomposition, the whole bulk of air employed ought to have been increased, which was not the case ; neither, on such a supposition, are we able to assign any probable reason why the production of acid should, for a time, have ceased at the precise period, when all the oxygen was consumed. We are, therefore, inclined to suppose a period, more or less long, to intervene between the formation of carbonic acid, during the growth of the seed, when oxygen gas is present, and its production by the same body, when the whole of that gas is consumed ; between that process which announces the evolution, and that which indicates the decomposition of the seed.

213. Should it, however, be objected to our experiments, that the process of decomposition may instantly follow the termination of living action, and thus confound the results, this objection will not apply to the experiments of M. de Saussure, who analyzed the residual air at intermediate periods of the process, and always found the quantity of carbonic acid formed, and of oxygen gas remaining, to make

together $\frac{21}{100}$ of the whole air employed. Thus, says he, if seeds be made to germinate in 100 cubic inches of air, which contain 21 parts of oxygen and 79 of nitrogen gas, it will be found, that, if the germination has produced 14 cubic inches of carbonic acid, there remain but seven cubic inches of oxygen in the residual air; or, if only seven cubic inches of acid are formed, there will then remain 14 of oxygen gas. The volume of oxygen consumed is, therefore, equal to that of acid gas produced; and this result as yet admits of no exception*. In such circumstances, the carbonic acid, which seeds are capable of affording under decomposition, cannot be supposed to mix with that formed in germination, unless we not only reject the evidence derived from identity of volume between the two gases, but admit the apparent absurdity of believing, that, in the same body, the act of decomposition can consist with the function of life. For these reasons we adhere to our former opinion, that, in germination, carbonic acid is formed only by the union of the carbon of the seed with the oxygen gas of the air, and that the whole of the oxygen that is lost is to be found in the carbonic acid produced.

214. But though, in germination, the conversion of oxygen gas into carbonic acid is by no one denied, yet by some it is maintained that a part only of this gas is so changed, and that another portion of it is *absorbed* by the seed. The vague and indefinite use of the word *absorption* has, perhaps, contributed not a little to obscure the reasonings on this subject,

* Recherches Chim. p. 10.

which renders it necessary for us to state the sense in which we employ that term. In every science, precision in the use of terms is essential to correct reasoning; but in physiology, which embraces the properties and habitudes both of living and inanimate bodies, this condition is more indispensably required; and the uniform observation of it demands scarcely less attention than the process of reasoning itself. Add to this, that physiology, besides possessing a language of its own, converses extensively with almost every branch of science, and is therefore perpetually exposed to the danger of ambiguity, from the transference and misapplication of terms. Hence it has happened that its doctrines and its language have inclined sometimes to mechanical, at other times to chemical, science, and, not unfrequently, they have assumed a metaphysical aspect.

215. But whatever meaning the mechanical or chemical philosopher may attach to the word *absorption*, the physiologist can, with consistency, employ it to express only that operation by which fluids are received into the body through a living organized structure, or system of vessels. Such an operation differs from the imbibition of fluids by inanimate bodies, which is sometimes named absorption: it differs also from the attraction of gaseous bodies by fluids or by solids, which often passes under the same name: and, finally, it differs from the ascent of fluids in capillary tubes, which, whatever be its cause, does not partake of the nature of a living action. In all our investigations, therefore, we have constantly used the word *absorption* in its proper physiologi-

cal sense, and have never employed it to express, what we conceived to be, either a mechanical or chemical action.

216. In this view of absorption, although water enters the seed at first, as we think, by simple imbibition, yet there is no evidence that gaseous fluids can be absorbed and decomposed, in the manner which many eminent chemists have supposed: neither, as we have argued (17.), can this oxygen be held to gain admission by the operation of chemical affinity. The only probable reason, in favour of the absorption of oxygen, was derived from the belief that more of that gas was lost than could be found in the carbonic acid produced: but as the facts adduced (207. 8. 9.) seem to demonstrate the fallacy of the experiments on which this opinion was founded, the inference deduced from them can no longer be maintained. Even those, if such there be, who may still be disposed to deny the identity of volume preserved in the conversion of these gases, must, we think, admit the approximation to it to be extremely near; and therefore, the portion of oxygen, supposed to enter the seed, must be so small as to be incompetent to produce the effects which have been assigned to it. And to contend that the oxygen of the air enters the seed merely from certain effects which it is supposed to produce, is to invert the ordinary rules of philosophizing, which require that this alleged agent be shewn to be present in that body, before we venture to describe its operation.

217. But Mr Acton endeavours to support an absorption of oxygen in germination, from the results

of experiments made on the growth of seeds in oxygen gas. For a few days, the volume of gas, in these experiments, diminished, and the oxygen was more or less completely changed into carbonic acid : hence it is inferred that oxygen gas is absorbed by seeds, and this to the amount, in some cases, of one-third of the whole gas employed *. To these experiments many objections occur ; for, in the first place, pure oxygen is not the gas in which seeds naturally grow, and no such diminution of volume attends their ordinary germination in atmospheric air. Moreover, a mere diminution is no proof of the absorption of oxygen, in the physiological sense of that term ; for, in Mr Acton's experiments, paste and other inanimate substances produced a similar diminution of oxygen, although no living power could then have been exerted. On the other hand, this diminution, when it does happen, appears to be accidental ; for in the experiments of Scheele and Cruickshank (6.) no sensible change of volume seems to have attended germination, even in oxygen gas. These experiments, therefore, are not only exceptionable in themselves, but are contradicted by others, and by the results of germination in atmospheric air. They are farther objectionable from not distinguishing between the operations of living and inanimate matter ; and, were it even allowed that oxygen was absorbed in these experiments, such a concession would not prove that it is equally absorbed in atmospheric air, where the process is in all respects natural, and the results are altogether different.

* Nicholson's Journal, vol. xxiii. p. 229.

218. The important experiments of Messrs Allen and Pepys, to which we have already referred, enable us to make another correction in our conclusions concerning the carbonic acid formed in germination. We before followed the opinion of M. Guyton, that this acid contains only one-fifth of its weight of carbon; but these gentlemen have found that the proportion of carbon comes much nearer to the determination of Priestley and Lavoisier, the former of whom made it one-fourth, and the latter $\frac{1}{3.57}$ of the compound. The average result of their experiments afforded $\frac{28.60}{100} = \frac{1}{3.49}$ of carbon. They ascertained also, with great precision, the specific gravities of oxygen and carbonic acid gases; finding a cubic inch of the former to weigh 0.3382, and a cubic inch of the latter 0.4726 of a grain, which weights, they observe, correspond almost exactly with those previously assigned by Mr Davy.

219. If, therefore, the quantity of carbon, given out by the seeds in our experiments (8. 9.), be recalculated on the suppositions that no change of bulk attends the conversion of oxygen gas into carbonic acid, and that the relative specific gravities of these two gases are such as is above stated, it will be found, that the quantity of carbon, existing in the acid formed by germination, is greater than we before assigned when proceeding on different data, being $\frac{1}{3.18}$ instead of $\frac{1}{4.44}$, as before stated (13.): and, consequently, its proportion approaches nearer to the determination of Allen and Pepys, who, as we have seen, make it to constitute $\frac{1}{3.49}$ of the compound.

220. But now that the specific gravities of oxygen

and carbonic acid, and the composition of this latter gas, seem to be determined with so much accuracy, we can have no difficulty in estimating the actual quantity of carbon which growing seeds afford in a given time, if we know the quantity of oxygen gas that is consumed: for as the bulk of acid produced is exactly equal to that of oxygen lost, the quantity of carbon given out by the seeds will be indicated by the excess of the weight of this acid gas over that of the oxygen. Thus in the experiment already related (8.), twelve peas were put into a jar with 16.5 cubic inches of air, of which 3.46 inches were oxygen gas. By the fourth day, the whole of this oxygen was consumed, and the radicles of the seeds were about an inch in length. Now, as 3.46 cubic inches of carbonic acid were likewise produced, which contain 0.465, or rather less than half a grain of carbon, such may be considered to be the quantity of that substance furnished by these twelve peas in four days; or one pea, in that time, yielded 0.039 of a grain of carbon. Hence the quantity of carbon yielded by seeds, before they become capable of replacing it by absorption from the soil, is extremely small. Dr Thomson observes, that, from a good many trials, made with as much care as possible, the quantity of carbon separated, during the whole process of malting, by the formation of carbonic acid gas, does not exceed two per cent *.

221. With respect to the mode in which the carbon of the seed unites with the oxygen gas of the

* Syst. Chem. vol. v. p. 446. 3d edit.

air, we have, at present, nothing to add to what has been already stated (16. et seq.). We have there argued, that no proof exists of any part of the air being absorbed by, or attracted into the seed; but have supposed that the combination of carbon and oxygen takes place exterior to its vascular structure.

ADDITIONS TO CHAP. II.

OF THE CHANGES INDUCED ON THE AIR BY THE VEGETATION OF PLANTS.

SECT. I.—*Of the Vegetation of Plants in the Shade.*

222. **T**HE necessity of water, heat, and air, to carry forward the process of vegetation, is universally admitted, and the general operation of these agents is abundantly established by the facts (23. 4. 6.) already related. In our former discussion of the specific changes induced on the air by vegetation (29. et seq.), we endeavoured to establish, by experiment, the same general facts as have been described to take place in the germination of seeds; but the facts, which are supposed to lead to an opposite conclusion, are so numerous, and, apparently, so decisive, as still to persuade many eminent chemists, that oxygen gas, instead of being consumed, is really produced by the ordinary process of vegetation. In the present chapter we propose, therefore, to reconsider our own experiments and opinions, and to examine, with more attention, those which are opposed to them, in order to discover, as far as we are able, the causes of these discordant results, and thus to arrive at as much truth

and certainty as our present knowledge of the subject will enable us to attain.

223. It will be seen, as we proceed in this discussion, that our opinions on this subject are by no means so novel as they have been commonly supposed, nor as we ourselves, at one time, thought them to be; and we shall gladly introduce the opinions of preceding writers, not only because they must add weight to our own, but because we are anxious to do entire justice to those who have gone before us in investigating this much controverted subject.

224. The experiments which have been detailed in the former part of our Inquiry (31. et seq.), and which were designed to prove the complete conversion of oxygen gas into carbonic acid by vegetation, were repeated a great number of times, and with every attention to guard against fallacy, which our own experience, and that of many friends who witnessed their progress, could suggest. In a subsequent repetition of these experiments, we varied the mode of analysing the residual air, by first raising the jar, and then passing the alkaline solution (32.) which contained the carbonic acid, into a tube filled with mercury, and inverted in a trough of that fluid. To this solution, a portion of diluted acid was then added, which excited a brisk effervescence, and caused the disengagement of more than two cubic inches of carbonic acid gas. On comparing the volume of this gas with the diminution which the whole air had suffered, we found it to be nearly equal; so that we thus obtained, in a distinct and palpable form, a bulk

of carbonic acid, corresponding, nearly, with the quantity of oxygen gas, which the air, employed in the experiment, actually contained.

225. There is, however, one possible source of fallacy in these experiments, which our friend Mr Murray, who repeatedly witnessed their progress, and has expressed himself fully satisfied of their apparent accuracy, has since suggested, to which it becomes us to attend. "The plants submitted to experiment, both in the experiments of Scheele and of Mr Ellis, were," he observes, "those the white and succulent stems of which are large in proportion to their leaves, such, for example, as peas and mustard. Now, it appears from the experiments of Ingenhousz and Saussure, that it is only by the green parts of plants that carbonic acid is decomposed and oxygen evolved; while from the white and succulent parts, it is established by Saussure, that carbonic acid is formed by the action of the oxygen of the surrounding air upon them *." A similar objection occurred to Dr Henry, who saw the experiments repeated in June 1807, and satisfied himself of their general accuracy in other respects, by obligingly taking the trouble to analyse the residual air.

226. In order to meet this objection, it is necessary to recur again to experiment, and to employ plants of a greater age, or such as have no white parts belonging to them. With this view, we procured several plants, which grew in small garden pots, and placed them to vegetate in given quantities

* Syst. Chem. vol. iv. p. 53. 1st edit.

of atmospheric air. To accomplish this purpose without injury to the plant, we caused several circular tin dishes to be made, some of which were about six inches in diameter, and one and a half in depth, and others of little more than half these dimensions. Each dish was divided through its middle into two parts, and exactly in the centre of each part a semicircular cut was made, so that, when the two parts were brought together, a circular hole was formed, just large enough to admit the stem of the plant. One of the parts was made a little smaller than the other, so as to pass about an inch within it; and to the bottom of this smaller part a flat piece of tin was partially fastened, in such a manner as to receive the bottom of the larger half in a sort of sheath, by which the two parts were rendered perfectly steady. As the plant grew in the pot, the two parts of the dish, previously separated, were brought together, one on each side the stem, and made to slide into each other without injury to the stem, which rose through the central opening,—while the circular dish itself now rested securely on the margin of the pot. By this arrangement, the plant was left growing in its natural situation, and all sources of fallacy from the mould were effectually removed. The junction of the two parts, and the aperture which received the stem of the plant, were now made water-tight by a proper luting, and the jar was then inverted over the plant into the tin dish, which was now filled with water.

227. One of these dishes was made to inclose a young bean as it grew in a pot, to the height of three

and a quarter inches, and, after the junctures were secured by luting, a small jar, of the capacity of 18.5 cubic inches, filled with atmospheric air, was inverted over it, and the communication with the external atmosphere was cut off by water poured into the dish. In this situation, the plant grew in a room, in full day light, from the 10th to the 14th of August, in a temperature varying from 60 to 65 or 68°. It increased about two inches in height during this period, and at length pressed against the upper part of the jar. The air of the jar was now examined. It did not contain any sensible portion of carbonic acid, which had, probably, been attracted by the water that had entered the jar; but 100 parts of the residue lost only 16 by slow combustion with phosphorus, so that the atmosphere had lost $\frac{5}{100}$ of its oxygen gas.

228. Another young and vigorous bean plant, growing in a pot to the height of seven inches, was confined, in a similar manner, in about 96 cubic inches of atmospheric air, a little more than half a cubic inch of the water of potassa being previously placed in an egg-cup underneath the jar. From the 11th to the 15th of August, the plant grew at the rate nearly of three quarters of an inch a-day, and the water, at the same time, rose gradually into the jar. On the 16th and 17th its growth was much less, and at last it was not perceptible. Some of the lower leaves of the plant now began to look dry and black. In the whole, it had grown in height more than two and a half inches, and the water, when the experiment was terminated, had risen one and a half inch into the jar.

229. By the former experiment (227.), it was proved that the oxygen gas of the air was in part consumed, and the same circumstance may be presumed to have happened in this case, from the gradual decline and final cessation of growth which took place. The formation of carbonic acid may also be presumed, from the continued rise of the water into the jar ; but to prove this more clearly, we raised the jar, and passed the alkaline solution into a small jar filled with mercury, and inverted in a bason of that fluid. A small quantity of diluted sulphuric acid was then added to the solution, which caused an immediate effervescence, and the extrication of nearly two cubic inches of carbonic acid gas, which were afterwards entirely attracted, with the usual phenomena, by pure lime water. These experiments therefore prove, that by the growth of young beans, which contain no white parts, but, on the contrary, have both leaves and stems of a deep green colour, the oxygen gas of the air is changed into carbonic acid, in the same manner as by the mustard plants mentioned (32.) in the former part of our Inquiry. Similar results were obtained by placing radishes, a plant of *hydrangia mutabilis*, and some others, in like circumstances. These experiments were likewise seen by Mr Murray, who considers them as obviating entirely the objection which he had previously suggested *.

230. To remove any objection that may be made to the early age of these plants, or to their being en-

* Syst. Chem. v. iv. p. 47. 2d edit.

tirely of an herbaceous nature, we next procured a young willow plant, about 14 inches high, the stem and greater part of whose branches were of a firm and woody texture. It grew in a garden pot, and a tin dish, of a large size, was adjusted to it, in the manner already related. On the 28th of July, a bell shaped jar, of the capacity of 250 cubic inches, was inverted over it, the mouth of which was, as usual, surrounded with water. The apparatus was set aside in a room, in full day light, but not exposed to the sun, where the temperature, through the day, varied from 60 to nearly 70°. The plant looked quite fresh and in a growing state for a few days, but on the 31st, two of its leaves fell off, and, by the 3d of August, nearly half its leaves had fallen. Apprehensive that if the leaves were suffered to remain within the jar, decomposition might ensue, and disturb the natural result, we removed them by immersing the whole apparatus in a deep trough of water, and then cautiously raising the jar, we drew away all the fallen leaves under water, without permitting any fresh air to enter. At the same time, a small quantity of the air of the jar was passed into another jar, and was afterwards found, on analysis, to contain no carbonic acid, but it lost $\frac{2.5}{100}$ by the slow combustion of phosphorus. The apparatus was afterwards replaced in its former situation.

231. During the following days, the leaves continued to fall, and by the 6th of August, few remained on the branches; the plant, however, in other respects, looked healthy, and had grown about an inch. We now again removed the fallen leaves as before,

and examined a second portion of the air, which, after washing in lime water, now lost only $\frac{13}{100}$ by the test of phosphorus. The plant was then restored to its former situation, and suffered to remain till the 18th of August, at which time its stem had reached the top of the jar, having grown in all nearly three inches. The extremities of its branches still looked thriving and succulent, but they were almost entirely defoliated. The atmosphere being now a third time examined, lost, in repeated trials, only $\frac{11}{100}$ by the combustion of phosphorus, so that $\frac{10}{100}$ of its oxygen had, in the whole, disappeared. In this experiment, the gradual deterioration of the air in which the growing plant was confined, cannot otherwise be explained than by supposing its oxygen to be converted into carbonic acid, which acid was attracted by the water, in which the vessel was inverted.

232. In the same manner, we placed on the 10th of July, a young and vigorous myrtle tree, seven and a half inches high, and growing in a garden pot, in 130 cubic inches of air which was confined by water. On the 18th, the plant looked fresh, but half a dozen leaves had fallen, and many more fell the next and succeeding days. On the 22d, the pot and jar were immersed in water, and a portion of its air removed; 100 parts of this air being shaken with lime water lost three parts, and the remainder lost by combustion with phosphorus 16 parts more, making in the whole $\frac{19}{100}$; so that $\frac{5}{100}$ of the atmosphere had been consumed, of which three existed in the form of carbonic acid, and the two other parts may be reasonably supposed to have been also attracted, in

the form of acid gas, by the water over which it stood, and through which it was passed. These experiments, therefore, prove, that ligneous, as well as herbaceous, plants, during their growth, equally convert the oxygen gas of the air into carbonic acid.

233. It is worthy of remark, that, in both these experiments with the willow and myrtle, the plants were almost entirely deprived of their leaves, a circumstance for which we cannot satisfactorily account. There was no want of moisture in the earth in which they grew, nor was there any substance in the jars to abstract their moisture, and thus to accelerate the fall of the leaves. The leaves, also, did not appear diseased, nor in any respect changed from their natural state; neither have we observed any thing similar to happen with herbaceous plants. There can be no doubt but that the atmosphere, in which the plants were confined, was supersaturated with moisture, which would certainly check evaporation from the surfaces of the leaves; but whether the stagnation thus created was the immediate cause of the phenomenon, we do not undertake to decide. The qualities of the atmosphere had not undergone, in other respects, any very great change, not above one-fourth of its oxygen having been destroyed in the case of the myrtle; nor, if this had proceeded to a greater extent, would it explain the fall of the leaves, though it might have occasioned the death of the plants. But neither of the trees, in this instance, died: on the contrary, they were growing, in a small degree, all the time, and, after being released from their confinement, they again recovered their leaves.

234. In the foregoing experiments, no circumstance, as far as we can discover, was present, which could interfere with the natural progress and result of vegetation. The plants remained growing in the earth, and except that they were confined in a small volume of air, every other circumstance was natural to them. Nor was any substance admitted into the glass vessels, except the plants, which could, in any material degree, affect the air, unless it be conceived that the small quantity of luting, employed to close the junctures, operated in that way. We found indeed, that the lutes we employed did, in a small degree, act on the air; but we satisfied ourselves that the small quantity used, compared with the volume of air employed, and covered, as it often was, with a considerable depth of water, could not, in any material degree, interfere with the expected results. To do away, however, the possibility of error from this cause, we varied the method of experiment in the following manner, in which no luting was employed.

235. We procured some young succulent cuttings of willow, and placed several of them together in phials of pure water, which phials were then confined under separate jars of atmospheric air, inverted in water. The jars were then set aside in the shade on the 9th of July. By the 16th of that month, the cuttings had thrown out small rootlets, which, by the 22d, were, in some instances, nearly an inch in length. On the 23d, the air in one jar was examined. It lost $\frac{7}{100}$ by agitation with lime water, and afterwards, by the slow combustion of phosphorus, it lost eleven parts more, making $\frac{18}{100}$ in all; so that nearly half the oxygen

gas of the air employed was consumed by vegetation, of which seven parts existed in the state of carbonic acid at the time of examination, and the remaining three parts may reasonably be supposed to have been attracted, in the state of acid gas, partly by the water over which the vessel was inverted, and partly by that through which it was passed, when submitted to examination.

236. On the 26th of July we raised a second jar, under which cuttings of willow had been confined, and in which, also, a small quantity of solution of potassa had been placed. Into this jar the water had risen half an inch, and, on pouring diluted sulphuric acid into the alkaline solution, a very brisk effervescence was excited. In a third jar, where a similar quantity of solution had been inclosed, the same phenomena were exhibited. In a fourth jar, $\frac{1.1}{100}$ of the oxygen gas were consumed. In a fifth, where nine similar cuttings of willow were placed in a large mouthed phial under a jar containing 73 cubic inches of atmospheric air, the air, on being examined on the 5th day, lost $\frac{1.1}{100}$ by lime water, which it rendered milky, and the residue lost seven parts more by the slow combustion of phosphorus. Allowing also a little acid gas to have been attracted by the water during the analysis, we are enabled to account for the whole oxygen which the air originally contained, of which $\frac{1.4}{100}$ had been converted into carbonic acid, and $\frac{7}{100}$ remained at the time the analysis by phosphorus was made. In these experiments, therefore, no substance, except the vegetables, was present, which could, in the smallest degree, affect the air; and the results correspond entirely with those already given.

237. We still farther varied these experiments, by taking up a young bean out of the pot in which it was growing, and carefully washing all the mould from its roots. The plant was about seven inches high, and its roots were put into a cubic inch glass measure, nearly filled with water, on the 20th August, when the thermometer was at 66° . The glass measure was then put into an egg-cup containing one cubic inch of the water of potassa, and the egg-cup was placed in a deep saucer. A bell-shaped jar, of the capacity of 86 cubic inches, containing atmospheric air, was then inverted over the plant, and mercury poured round its mouth, to cut off the communication with the external air; a small stratum of water, about $\frac{1}{10}$ of an inch deep, being poured on the surface of the mercury. By the 21st, the plant had grown more than one-fourth of an inch above a thread which had been tied round the jar to mark its height, and the mercury had risen a little into the jar. By the 22d, the mercury was about $\frac{3}{10}$ of an inch high in the jar, and the plant was still growing. By the 23d, the mercury stood $\frac{5}{10}$ of an inch high in the jar, and notwithstanding the film of water that covered the mercury, the lower leaves were much discoloured. We therefore put an end to the experiment, by raising the jar out of the mercury; and then passing the alkaline solution into a small jar of mercury immersed in a bason of that fluid, we obtained, by adding to it a due quantity of diluted sulphuric acid, about one cubic inch of carbonic acid gas. Now, the diminution in the volume of air, in this experiment, must have

arisen from the attraction of carbonic acid by the alkaline fluid, and this carbonic acid must have been in part formed out of the oxygen gas of the atmosphere; for had it been formed by the plant, independently of the surrounding atmosphere, no diminution of volume would have taken place.

238. Several cuttings of willow were, likewise, placed in a large-mouthed bottle filled with water, and confined in about 85 cubic inches of atmospheric air, from the 20th to the 24th of August. The mouth of the inverted jar was surrounded by mercury, and it was set aside in day light, but not exposed to the direct rays of the sun. One hundred parts of this air lost five by agitation with lime water, and thirteen parts more, by the slow combustion of phosphorus; so that the three remaining parts, required to make up the $\frac{21}{100}$ of oxygen gas, which the air originally contained, must have been converted into carbonic acid, which was in part attracted by the film of water that covered the mercury, and partly by the water through which it was passed when submitted to examination.

239. The whole of the experiments which have been now detailed appear to us to establish, in a satisfactory manner, the conclusions which, in our former publication (32. 3. 4.), we ventured to draw; for they prove that the oxygen gas of the atmosphere is converted into carbonic acid gas by the process of vegetation, and that the bulk of the latter gas nearly or exactly corresponds with that of the former; and, consequently, they demonstrate that the air is deteriorated by the growth of plants, in the same manner

as by the germination of seeds, and that no part of the oxygenous portion of the atmosphere combines with the substance of the plant.

240. It may, perhaps, add weight to the foregoing conclusions, if we are able to shew, that, notwithstanding the apparent contrariety of fact and opinion which has prevailed on this subject, the writings of various authors furnish decisive evidence that growing vegetables produce in the air those specific changes, which we have held to be essential to vegetation. Even so far back as the time of Dr Hales, an experiment is related which entirely accords with these views. That eminent philosopher set a plant of peppermint, on the 29th of June, in a glass cistern of earth, and filled it with water. Over this cistern he inverted a glass vessel, and then drew out part of the air by a syphon, so as to leave 49 cubic inches of air in the vessel. By the side of this vessel he placed another of the same size, inverted in a similar manner, but without any plant beneath it. In a month, the mint in the first vessel had put forth several young shoots. The water, in both vessels, rose and fell with the variations in atmospheric pressure and temperature; but the water, in which the peppermint stood, rose so much above its first fixed station, and above that in the other vessel, that one-seventh part of the air must, says Dr Hales, have been reduced to a fixed state. This happened chiefly in the summer months. In the beginning of the following April, the old mint was taken out, and a

fresh plant put in its place, to try if it would imbibe any more of the air, but it faded in four or five days ; and yet a fresh plant, put into the other vessel, whose air had been confined for the same time, lived nearly a month, or almost as long as another plant lived in newly confined air *.

241. This great diminution of the air of the vessel in which the mint had grown, Dr Hales supposed to afford a proof that the leaves and stems of plants imbibe elastic air ; but we now know that carbonic acid must have been formed, and afterwards attracted by the water over which the vessel was inverted. That the pure part of the air had, in a great measure, disappeared, may be inferred, also, from its incapacity to support a fresh plant that was placed in it, which the air, confined the same time in a similar vessel, was found able to do. Dr Hales himself, indeed, proved this fact by an analysis of the residual air ; for he found, that, after being infected by the mint, so that a fresh plant would not grow in it, it suffered a farther diminution of four cubic inches, when a mixture of sulphur and iron-filings with water was placed in it. If, therefore, we consider the first diminution of the air by the plant to have been occasioned by the formation and attraction of seven cubic inches of carbonic acid (which is one-seventh of the whole air employed), and add to it four cubic inches of oxygen gas, abstracted by the mixture of iron and sulphur, we shall have eleven cubic inches as the whole loss which the air suffered ; and $\frac{11}{49} = \frac{1}{4\frac{1}{5}}$, we now know

* Veg. Statics, vol. i. p. 329, 1st edit.

to be nearly the proportion of oxygen gas which the atmosphere contains.

242. We have already given (27.) the results of Dr Priestley's experiments on this subject, and have shewn them to be so much at variance with each other, as altogether to preclude the possibility of any just inference being drawn from them. It may, however, be useful to examine more minutely the circumstances in which these experiments were made, since we may thereby be led to a discovery of the causes of their discordance, and a detection of some sources of error, which the state of chemical science did not, at that early period, enable this candid philosopher fully to appreciate.

243. Dr Priestley had himself at first expected, that, "since common air is necessary to vegetable as well as to animal life, both plants and animals would have affected it in the same manner;" but he found, that, after a sprig of mint had grown for some months in a glass jar, standing inverted in a vessel of water, the residual air was still capable of supporting combustion, or respiration. He observes, however, that, in this experiment, the root of the mint decayed, and the stalk also, beginning from the root, and yet the plant continued to grow upwards through a black and rotten stem: and in this manner a sprig of mint lived, the old plant decaying, and new ones shooting up in its place, all the summer season*.

244. But we now know, that, under the decomposition of vegetable bodies, various gases are disen-

* Obs. on Air, abridged, vol. iii. p. 250.

gaged, and, among these, oxygen gas itself may be either directly or indirectly set free. Thus M. de Saussure confined two plants of *mentha aquatica* in two equal jars of common air, and placed them to vegetate in sunshine for three weeks. By the side of each plant he put a piece of withered dead mint; but in one case the mint was covered by water, while, in the other, it was suspended in the air of the vessel. The plant, which vegetated in the neighbourhood of the suspended mint, had not ameliorated its atmosphere, but that, which grew in the vessel with the moistened dead leaves, had added many times its volume of oxygen gas to the common air which surrounded it *. Dr Priestley himself, indeed, observes, that, “in repeating his experiment, care must be taken to remove all the dead leaves from about the plant, lest they should putrefy, and affect the air †; and yet he inadvertently draws his conclusion from an experiment where this process of putrefaction was constantly going on.

245. There are other experiments, however, of this philosopher, made in a less exceptionable manner, and to which no apparent objection occurs. He had several instances, he says, of vitiated air being meliorated, and of common air being considerably improved, by the shoots of strawberries, and of some other plants, which he could, by bending, introduce into jars of air, while the roots continued in the earth. This he considered to be the fairest method of trial,

* Recherches Chim. sur la Vegetation, p. 229.

† Obs. abridged, vol. iii. p. 251.

as the plant grew, in every respect, in its natural way, except that part of the stem was obliged to lie in water, and the shoot was in air confined in a narrow jar *. All the cases in which common air was improved by vegetation, he elsewhere says, were those in which the roots of the plant were in the ground, and flexible sprigs from them were bent, and made to pass, in this manner, through water into the jars containing the air †. Most of these experiments, however, were made in June and July, and, as they were conducted in the open air, it is highly probable that they were carried on under a direct exposure to the rays of the sun, which we shall hereafter see to exert peculiar effects on the gases produced in this process.

246. Several sources of fallacy exist in the experiments which Dr Priestley made to restore the purity of air that had been vitiated by the combustion of wax, tallow, and alcohol. He placed sprigs of mint, and other plants, in portions of this air; and he found that five or six days were sufficient to restore the air, when the plant was in its vigour. He farther considered this restoration to depend on the *vegetating state* of the plant; for when the fresh leaves only of mint were kept in such air, for a long space of time, and were frequently changed, no melioration could be perceived in it ‡.

247. On these experiments we may remark, that combustion generally terminates in closed vessels

* Obs. on Air, vol. iv. p. 299. † Ibid. p. 307.

‡ Obs. on Air, vol. iii. p. 252. 253.

some time before the whole of the oxygen gas is consumed ; and, consequently, enough of that gas might remain to support vegetation, especially that of the hardier plants. But a still greater source of fallacy is, probably, to be found in the carbonic acid formed in these processes, which, if retained in contact with the plant, might, as we shall hereafter see, be decomposed by Light, and thus furnish oxygen gas to carry on the growth of the plant. It is worthy of remark, that Mr Scheele never found foul air to be rendered salubrious by vegetables, even when placed in the light of the sun ; but, in all his experiments, he carefully removed the carbonic acid by washing it in milk of lime before he placed the vegetables in it *. And, in accordance with this fact, Dr Priestley himself observes, that when he employed air that had been vitiated by iron-filings and brimstone, or by nitrous gas, it did not fail to kill the plant †.

248. Dr Priestley's next attempts to establish the purifying powers of plants were made on air that had been injured by animal respiration. Into a jar, nearly filled with air rendered noxious by mice dying in it, he put a sprig of mint, on the 20th June ; and another portion of the same air he put into a phial, which was placed by the side of the former. In seven days, a mouse lived five minutes without uneasiness in two and a half ounces of the air in which the plant had grown ; but died in two or three seconds, in the same quantity of the air of the other phial.

* On Fire and Air, p. 37. 163.

† Obs. on Air, vol. iv. p. 301.

In a similar quantity of common air, another animal lived seven minutes; and Dr Priestley concluded, “that the restored air wanted about one-fourth of being as wholesome as common air*.” In another experiment, a fresh mouse lived 14 minutes in two and a half ounce measures of respired air, that had been restored by vegetation; and in air, rendered incapable of supporting the flame of a candle by Dr Priestley’s own respiration, he also found, that a candle would again burn, after a sprig of mint had been made to grow in it. In this case too, the effect, he adds, was not owing to any virtue in the leaves, but to the vegetating powers of the plant†.

249. In these experiments, the same sources of fallacy may be pointed out as in those made on air injured by combustion. No precaution was taken to remove the carbonic acid formed by the respiratory process, nor is it stated whether the experiments were conducted in sunshine, or in the shade. The higher orders of animals, also, die in confined gas some time before the whole of the oxygen is consumed, so that vegetation might, to a certain extent, go on in air incapable of supporting animal life.

250. Dr Priestley, in the last place, endeavoured to restore the purity of air that had been injured by the putrefaction of vegetable and animal bodies. His general hypothesis required that vegetables should continue to live, and that animals should die, in such vitiated air; but he observes that insects, of

* Obs. on Air, abridged, vol. iii. p. 266. 7.

† Ibid. p. 268.

various kinds, live perfectly well in tainted air, which would have instantly killed other animals. He has even been obliged to take plants out of such putrid air, on purpose to brush away the swarms of insects that infested them, so fast did they multiply in these circumstances. Not only, however, did animals thus live in air, in which, on Dr Priestley's hypothesis, they might have been expected to die, but vegetables died in that in which they ought to have lived; for he found, that "when air had been freshly and strongly tainted with putrefaction, sprigs of mint have presently died on being put into it, their leaves turning black; but if they did not presently die, they throve in a most surprising manner *." From the facts already stated (244.), it appears that the air, vitiated by putrefaction, is restored, in certain circumstances, to a state capable of supporting vegetable and animal life, and inattention to these circumstances suggests at once a probable reason for that contrariety of result which these experiments afford.

251. Towards the end of the year 1771, continues Dr Priestley, these experiments did not answer so well, the restored air having relapsed to its former noxious state. In 1772, he had again the most indisputable proofs of the restoration of putrid air by vegetation; but, in 1778, the experiments were unfavourable, for whether they were made with air injured by respiration, by combustion, or by any other means, the air was not rendered better, but worse,

* Obs. on Air, abridged, vol. iii. p. 262.3.

by vegetation ; and the longer the plants continued in it, the worse it became. As the general result of all his experiments, however, Dr Priestley thought it *probable*, that the vegetation of healthy plants, growing in situations natural to them, had a salutary influence on the air in which they grew, since one clear instance of the melioration of air in these circumstances, should, says he, weigh against a hundred, where the air is made worse by it*.

252. Such is the series of experiments from which Dr Priestley endeavoured to shew that plants, by their vegetation, purify corrupted air : and such are the terms in which his opinion is finally expressed. The *probability* only which he attached to this opinion, has, by others, been converted into *certainty* ; and time hitherto has appeared rather to confirm, than to correct the error. The contrariety, however, that pervades all his experiments, and the “ fearful odds” against which his general conclusion is drawn, are, of themselves, sufficient to beget doubt and distrust ; and if, to these circumstances, we add the numerous sources of fallacy which have now been stated, and the want of precision in the methods of analysis employed †, we must be convinced that

* Obs. on Air, abridged, vol. iii. 265. 273. 275.

† Dr Priestley did not discover the use of nitrous gas, as an eudiometrical test, till the close of the year 1772, when his experiments on plants in that year were nearly brought to a conclusion. Previous to this period, he employed the burning of a candle, and the respiration of mice, as tests of the purity of air* ; and it is worthy of remark, that his experiments in 1778, when his methods of analysis were improved, totally contradict those of an earlier period, when they were confessedly imperfect,

* Obs on Air, vol. iv. p. 303.

these experiments are altogether inadequate to decide the general issue of the present question.

253. Lastly, Dr Priestley relates that his method, in making these experiments, was to put the roots of the plants into phials filled with earth and water, and then to introduce them, through water, into the jar containing the air on which he was making the experiment,—a method which itself gives rise to new sources of error. It was observed long since by Dr Ingenhousz, that fresh vegetable mould, confined in common air, considerably depraved it, by forming, as he supposed, carbonic acid *. Dr Thomson confirmed this fact, by finding, as M. Humboldt had also done, that newly turned up soil causes a removal of the oxygen gas of the air, and a production of carbonic acid †. And M. de Saussure found that pure earth, confined either in atmospheric air, or in oxygen gas, formed carbonic acid in proportion to the oxygen lost, without at all diminishing the volume of the air, farther than by the subsequent attraction of a small portion of the acid. The oxygen consumed, he adds, is found in a quantity rigorously equal in the carbonic acid produced: whence it follows, that the earth does not assimilate the oxygen to itself, but only affords carbon to unite with it ‡. From these facts, it is evident that the earth, in Dr Priestley's experiments, must have continually vitiated the air, and thus have introduced a constant source of fallacy into their results.

* Exper. sur les Veget. tom. ii. p. 189.

† Syst. Chem. vol. iv. p. 458. 1st edit.

‡ Recherches Chim. p. 179.

254. We have been drawn into these detailed remarks, not from any desire to depreciate Dr Priestley's labours, but from the circumstance of their having first given origin to the opinion, that plants, by their vegetation, at all times purify the air, and from a consideration of the importance which has ever since been attached to them. In the experimental sciences, it is chiefly by the successive detection of each other's errors that we gradually advance to truth ; for rarely, indeed, does it happen, that human sagacity can at once foresee and appreciate all the possible circumstances in an experiment, which may influence and controul its result. There is, therefore, no cause to wonder that this illustrious philosopher did not discover those sources of fallacy, which the more advanced state of science has alone enabled his successors to point out ; and the reflection, that our apparently more correct views may, at no distant day, undergo a similar revision, ought not only to teach us becoming diffidence in our own opinions, but may serve to check that rising triumph, which little minds are sometimes apt to feel, when they see thus exposed the mistakes of superior men.

255. The accurate and decisive experiments of M. Scheele, on the germination of seeds, have been already detailed (207.), and his experiments on the vegetation of plants seem to have afforded precisely the same results. Dr Priestley, he observes, found that vegetables make foul air salubrious ; but, in his own experiments, they always injured the air. He kept vegetables in a closed matrass, filled with foul air, both in darkness, and in the light of the sun,

and examined some part of this air every two days, but he always found it foul * ; so that the experience of Scheele is directly opposed to that of Priestley.

256. From what has been stated, however, we may collect, that both these eminent men were, to a certain extent, correct, and that both erred only by pushing their conclusions too far. Little doubt can remain but that, in many of Priestley's experiments, the air was much ameliorated ; and it is equally credible that, in all those of Scheele, it continued unimproved. Both philosophers seem to have been aware how much the state of the plant, and the agency of light, influenced the result ; but both failed in duly discriminating the composition of the air, which they were accustomed to denominate foul ; for while the air used by Priestley (247.) seems always to have contained a portion of carbonic acid, that employed by Scheele (247.) consisted only of nitrogen gas. It was this difference in the composition of the air employed in the experiments, which gave rise to a difference in their results ; and the mode in which it contributed to do so will hereafter be distinctly stated.

257. The experiments of Priestley and Scheele were repeated, and pursued to a great extent, by Dr Ingenhousz. Common air, says this author, is equally necessary to the life of animals and of plants. It is, however, only one-fourth part of this air that serves this purpose ; and hence a plant, confined in

* On Air and Fire, p. 163.

they were confined, was diminished one cubic inch and a half: its purity, also, was greatly impaired; and it contained $\frac{6}{100}$ of carbonic acid. Experiments made with plants of mint, in the shade, afforded similar results; but when the apparatus was exposed, for the same time, to the direct agency of the solar rays, the air experienced no change, either in its purity or its volume. From these and other experiments, M. de Saussure concludes, that plants, like animals, continually form carbonic acid, when they vegetate in atmospheric air, either in the sunshine or in the shade; that the oxygen of the air contributes to the formation of this acid; and that the reason why carbonic acid is not detected in the apparatus exposed to the sun, is, because it is then decomposed as fast as it is formed *.

261. In subsequent experiments, relating to vegetation †, M. de Saussure directed his attention almost entirely to ascertain the influence of solar light in decomposing carbonic acid. With this view he confined plants in mixed atmospheres of common air and carbonic acid gas, and exposed the apparatus to the direct rays of the sun, whereby he obtained very different results. We shall, therefore, postpone our report of these experiments to a future occasion, observing, for the present, that the experiments related above, accord entirely, in their results, with our own, and with those of other writers, respecting the effects produced in atmospheric air, by plants which vegetate in the shade.

* Annales de Chimie, t. xxiv. p. 139. et seq.

† Recherches Chim. sur la Vegetat.

262. To the experiments of Dr Woodhouse on the vegetation of plants in atmospheric air, we have already referred (30.), and have stated their general agreement with those which have now been related. He found that plants, confined either in atmospheric air or in oxygen gas, previously washed in lime water, rendered it impure, by forming carbonic acid. He farther asserts, that growing vegetables do not purify the air, but that the same effects are produced in the air by the growth of plants, as by the germination of seeds, and from the same causes. He supposed, indeed, that the carbon, which united with the oxygen, was afforded, not by the living plant, but by a dead portion of the leaves *. The facts, however, which have been so fully stated, sufficiently evince that this acid may be formed by the living, as well as by the dead, vegetable substance.

263. From the series of experiments and opinions which has now been delivered, we see that all the more distinguished philosophers, who have applied themselves to the investigation of this subject, with the single exception of Dr Priestley, agree in the following general facts: That oxygen gas is, at all times, necessary to vegetation; that, in the shade, or in perfect darkness, this gas constantly disappears, and is replaced by carbonic acid; and that, when the atmosphere contains no oxygen gas, vegetation no longer goes on. Even the experiments of Dr Priestley cannot be considered as opposing any serious objection to these conclusions; for though it were grant-

* Nicholson's Journal, vol. ii. p. 152.

ed, that, in certain circumstances, growing vegetables purify the air, yet such concession would not destroy the fact, that, in other circumstances, they cause its deterioration; and the imperfections, and contrarieties, which have been demonstrated in the experiments themselves, altogether annihilate their authority in establishing the general conclusion, which he has sought to deduce from them.

264. But in the experiments, which we before published (32. 3. 4.), we endeavoured to shew, from a careful observation and analysis of the air before and after the experiment, not only that a substitution of carbonic acid for oxygen gas took place in vegetation, but that the bulk of acid produced corresponded nearly, if not exactly, to that of oxygen gas which disappeared. In the experiments, also, which have now been related (236.), where the air was examined before the oxygen gas was entirely consumed, the bulk of acid abstracted by lime water, and the bulk of oxygen gas removed by phosphorus, composed together almost exactly the absolute quantity of oxygen which the atmosphere originally contained; which coincidence appears to us decisive, not only of the conversion of the oxygen gas into carbonic acid, but proves, farther, that none of this gas is retained by the living plant. We may add, that the results obtained by Hales, by Scheele, by Ingenhousz, and by Senebier, concur, more or less completely, to establish the same position, which is still farther fortified by the analogy derived from the case of seeds (208.), and by the inconsistency of supposing, without any better evidence than conjecture, that, while

the greater part of the oxygen is converted into this acid, a very small portion should still be retained by the plant.

265. But, perhaps, some who admit the conversion of oxygen gas into carbonic acid by plants growing in the shade or in darkness, may still contend that this is not a perfect vegetation, because the direct influence of light is withdrawn. We acknowledge the great influence of light in this process, but we have seen (25.) that it is not essential to it; for plants live and grow in situations from which light is wholly excluded. All the experiments also, which we have heretofore detailed, both in this and in our former treatise, although they were not made under a direct exposure to the sun's rays, were conducted in open rooms where light had the freest access; and the plants assumed all the characteristic properties and appearances, which were peculiar to them. It is, likewise, sufficiently evident, that, even in our own climate, and especially in high northern latitudes, a vast number of plants live and flourish in natural situations, where the direct rays of the sun seldom or never penetrate; and yet, in such situations, they attain a state of perfect vegetation. How many hours, also, of our brightest days, and even how many entire days, are we deprived of the direct influence of the sun's rays, at the very season when vegetation is advancing with the greatest rapidity and vigour? If, indeed, this direct influence were essential to vegetation, many plants, which we now behold, would never be produced at all, and all the tribes of vegetables would experience such frequent and continued

checks to their growth, that, in our own climate at least, we could scarcely ever hope to see many of them attain to a state of maturity.

266. Should we even allow, to the fullest extent, that plants, vegetating under direct exposure to the sun, produce changes in the air different from those which they produce in the shade, yet this, surely, does not disprove the correctness of our former observation. It only proves, that, by a change of circumstances, a change in effect is induced ; and as this effect is not essential to the end we desire to obtain, it must be considered as extrinsic and accidental ; for that condition can, at no time, be held necessary to the production of a given effect, which may, at any time, with impunity be excluded.

267. After what has just been observed concerning the changes induced on the air by vegetables growing in the shade, and in perfect darkness, it is almost superfluous to add, that similar effects must be produced by plants which vegetate through the night. Dr Priestley very early observed, that a fresh cabbage leaf greatly deteriorated the air in which it was confined for a single night, although it had not acquired the least smell of putrefaction * ; and Ingenhousz likewise remarked, that the more vigorous the plants were, the more was the air affected. He found that a withered plant, confined in air through the night, produced in it scarcely any change ; while a similar plant, in a state of active vegetation,

* Obs. on air abridged, vol. iii. p. 251.

diminished the oxygen gas of the air, and formed carbonic acid *. Many facts of a similar kind are to be met with in different parts of his work †.

268. With the view of confirming these facts, we placed some young mustard plants in a jar containing 40 cubic inches of atmospheric air. They were supported on a small whalebone hoop, covered with netting, beneath which a small cup, containing water of potassa, was placed. The jar stood inverted in a saucer of mercury; and at eight o'clock in the evening, when the temperature was 60° Fahrenheit, it was set aside in a dark room. By seven o'clock the next morning, the mercury had risen three-tenths of an inch into the jar; and, on raising the jar and pouring diluted acid into the alkaline solution, a brisk effervescence was excited, proving, in the usual manner, the conversion of the oxygen gas of the air into carbonic acid. In many subsequent observations on the vegetation of young beans in jars of atmospheric air, we likewise remarked their growth in the night, when the temperature was sufficiently high, to be little inferior to that which they make in the day;—a fact which is exemplified equally in the warmer regions of the globe, where a rapid succession of crops is obtained, and in high northern latitudes, where the short season of summer permits the intervention only of a few weeks between the periods of seed-time and of harvest.

* Exper. &c. t. ii. p. 117. 118.

† Ibid. vol. ii. p. 45. 46.

269. ALL that has been hitherto said, with regard to the changes induced on the air by plants, bears an immediate reference to the agency of the leaves, and of other green parts, in effecting these changes. But other parts of living vegetables produce, likewise, changes in the air, and these, in the next place, we propose shortly to consider.

270. Dr Priestley confined a full-blown rose in about eight cubic inches of atmospheric air, contained in a small glass jar inverted in water. On examining the air the next day, by the test of nitrous gas, he found it to be much depraved; and, by the following day, it was still farther depraved, but the flower, when withdrawn on the third day, did not seem to have lost any of its fragrance *. M. Scheele found, that, by confining fresh roots, fruits, herbs, flowers, and leaves, in separate vessels of common air, one-fourth part of the air was changed, in a few days, into carbonic acid gas †.

271. Dr Ingenhousz placed some flowers of marigold (*calendula*) beneath an inverted vessel of atmospheric air, and left them all night in his chamber. The next morning, a portion of the air was examined, and found to be incapable of supporting flame; it suffered, also, but little diminution when mixed with nitrous gas. He then exposed the vessel, with the remain-

* Obs. on Air, vol. iv. p. 311.

† On Air and Fire, p. 150.

ing part of the air, to the sun, for several hours, and found it, on examination, to be still more deteriorated. The flowers of honeysuckle produced similar effects, as did many others which he submitted to experiment *; so that flowers equally deteriorate the air in sunshine and in the shade.

272. In his experiments on flowers, M. de Saussure found that none, not even those of aquatic plants, could be developed in nitrogen gas; that the buds just ready to expand were suddenly checked, and those already developed speedily died. He considers flowers to displace in part the air that surrounds them, and to replace, by carbonic acid, the portion of oxygen which they consume. Those which are recently gathered, and confined in atmospheric air, do not sensibly alter its volume. The flowers which he employed did not suffer during the experiments; they had not lost their form or freshness, and many, which were only in bud, became fully expanded. The petals consumed more oxygen in sunshine than in the shade; but he tried in vain to excite detonation in oxygen gas, in which he had caused bunches of *fraxinella* to vegetate for eight days; and he thinks, therefore, that the inflammation, which may be excited in the atmosphere of this, and of some other plants, is owing to the combustion of an essential oil, and not to that of hydrogen gas †.

273. The changes thus induced on the air by the

* *Exper. sur les Veget.* t. i. p. 270.

† *Researches, &c.* p. 125. 129.

flowers of vegetables were supposed by Dr Darwin to be necessary to the elaboration and perfection of their sexual organs, and to the production of fruit. Each petal of the corolla of a flower has, according to him, a vessel which conveys its juices to the extremity, where they undergo a change of colour, as is seen in some party-coloured poppies. From the extremities of the petal, the juices are again returned, as in the leaves, by other vessels, and employed for the sustenance of the stigma, anthers, &c. This office of the petals may be inferred, he adds, from their vascular structure being visible to the eye; from many plants putting forth their flowers in autumn, before any leaves appear; and from others completing the process of impregnation early in spring, before their green foliage, or even their floral leaves, come forward; lastly, from the white petals of the christmas rose (*helleborus niger*) changing to a deep green after the seeds have grown to a certain size, and the nectaries, stamens and stigma have dropped off,—facts which shew, that the first structure was necessary to the production of the honey, wax, and pollen, but was no longer required after these were formed*.

274. With respect to fruits, Dr Ingenhousz observes, that, whether they were ripe or unripe, whether exposed in sunshine or placed in the shade, they always vitiated the air in which they were confined. His experiments were made on peaches, lemons, pears and apples, and the results were invariably the same†. M. de Saussure, however, found, that

* Physiologia, p. 51.

† Exper. vol. i. p. 274.

green sour grapes, while growing on the vine, and exposed to the sun in closed vessels, ameliorated common air without producing carbonic acid, and ripened in fifteen days; but when the vessels contained lime, they vitiated their atmosphere, in the same circumstances, and did not ripen. Grapes, apples, and pears, when separated from their respective trees, and placed in vessels of common air exposed to the sun, have vitiated their atmosphere in twenty-four hours, and produced carbonic acid; and when the experiment was made in perfect darkness, the acid produced was equal in bulk to the oxygen that had disappeared *.

275. Many experiments on the fresh roots of several plants were made by Dr Ingenhousz, who found them, in like manner, to deteriorate the air †; but as the roots were all immersed in vessels filled with water, these experiments are not so unexceptionable as those which he made on flowers. He elsewhere observes, however, that if a carrot, or the root of any other plant, be drawn fresh from the earth, and exposed, with its leaves, in a vessel of common air to the sun, the air will soon be manifestly deteriorated; but if the root be placed under water, and the foliage only be left in the air, the deterioration produced by the root will soon be repaired by the leaves ‡.

276. But the most precise information on this

* Recherches, &c. p. 129.

† Exper. t. i. p. 274.

‡ Ibid. t. ii. p. 38.

branch of the subject is afforded by the experiments of M. de Saussure. He observes, that a small portion of oxygen gas exists in the earth, and he was desirous to know whether the contact of this gas with the roots of plants was useful to vegetation. He therefore took several tubulated recipients, and, filling them with distilled water, set them to stand in separate basons of mercury. Having then drawn some young chesnut trees out of the earth, he put the roots of one tree through the mouth of each vessel, so that they should be entirely in the vessel, while the leaves were in the open air ; the mouth of each vessel was then carefully closed by a lute applied to the stem of the plant. Through the bottom of each recipient, a given quantity of gas was next introduced, which caused the water to fall, until the extremities only of the roots were immersed in it, while their greater part was exposed to the immediate contact of the gas employed.

277. In this manner he exposed the roots to nitrogen, hydrogen, and carbonic acid gases, and likewise to atmospheric air. The plants, whose roots were in contact with carbonic acid gas, died about the seventh or eighth day ; those which had their roots exposed to nitrogen and hydrogen gases died at the same time, which was about the thirteenth or fourteenth day : while three separate plants, whose roots vegetated in contact with atmospheric air, were still vigorous at the expiration of three weeks, when an end was put to the experiment. He adds, that the nitrogen and hydrogen gases suffered no change, but the bulk of air was increased by

the addition of carbonic acid furnished by the roots ; while the atmospheric air was diminished from the attraction of the carbonic acid, either by the roots themselves, or by the water. From these experiments, and from other considerations, M. de Saussure concludes, that the contact of oxygen gas with the roots of plants is useful to vegetation *.

278. Lastly, M. de Saussure confined the woody stems of plants, immediately before the appearance of their buds, in vessels of common air, with sufficient water to carry on their growth, and they came into leaf like those in the open air ; but in nitrogen and hydrogen gases they were unable to make this development, and perished without exhibiting any sign of vegetation. When deprived of their leaves, the ligneous stems of plants vitiate common air, either in sunshine or in the shade, without changing its volume ; and replace, with an equal bulk of carbonic acid, the oxygen gas which they make to disappear. The stems of the willow, the oak, the poplar, the apple and the pear, were employed in these experiments.

279. These effects which the flowers, the fruits, the roots, and the stems of plants produce in the air, at all times, and in all situations, accord completely with those which the leaves and other green parts of vegetables produce in the shade, since they all, more or less, convert the oxygenous portion of the air into an equal bulk of carbonic acid. These facts afford,

* Recherches, p, 104. 106.

therefore, additional evidence of the power of the vegetable to produce this specific change in the air, and shew that, with respect to these parts at least, it is the natural effect of vegetation. As, however, carbonic acid is thus, at all times, produced by these several parts of plants; and as all the green parts have likewise been shown to produce a similar effect in the shade; it is evident, that, if a contrary effect arise when the green parts are exposed to the rays of the sun, it must proceed from the peculiar agency of the sun upon those parts alone. We proceed therefore, in the next place, to investigate the changes induced on the air by the leaves and green parts of living vegetables, when exposed to the direct agency of the solar rays.

SECT. II. *Of the Vegetation of Plants in Sunshine.*

280. IN our former publication we were led, by the results of our own experiments, to consider the consumption of oxygen gas, by living plants, as the natural and necessary effect of vegetation; and this conclusion has, we presume, been abundantly confirmed by the additional facts and circumstances which have just passed under our review. But the opinion, which maintained the production of this gas by growing vegetables exposed to the sun, appeared so directly opposed to these views, that we were induced to question the accuracy of the facts, and to combat the correctness of the reasoning on which it rested;

and while, in certain circumstances, we admitted (45.) this production of oxygen, we denied (46.) that it either was or could be considered a necessary function of vegetation. A more extended inquiry into the original facts, together with the additional light, which the late researches of M. de Saussure have afforded, have contributed to raise our view of the importance of this subject; and therefore, in the present section, we have submitted it to a fuller and more distinct discussion.

281. On several occasions, we have already alluded to the operation of light in modifying the changes which living vegetables produce in the air; and we have also noticed the remarkable influence which it exerts in the production of the colours of plants. As we now propose to enter more fully into the nature of its operation on the vegetable kingdom, we shall briefly recite such additional facts and observations, relating to its more general effects, as have since come to our knowledge.

282. We before ascribed (25.) to M. du Fay the first observation respecting the direct agency of light in changing the colours of plants; but a more extended inquiry has taught us, that we must go much farther back to collect the first notices which relate to this subject. According to M. Senebier, Aristotle, in his theory of colours, remarks, that when water and the solar rays act together on plants, they produce a green colour; and, in conjunction with earth, the same water produces whiteness. Hence all those parts of plants which lie beneath the soil, as roots and bulbs, appear white, while the parts,

which are exposed to the sun, exhibit a green colour*.

283. The celebrated Ray possessed still clearer notions on this subject. He remarked that plants continued green while they vegetated under a glass bell exposed to the light; but when the light was excluded, by covering them with an opaque vessel, they lost their green hue, and acquired a whitish yellow colour. Their stalks, at the same time, became long, slender and feeble, and their leaves small. The cause of the green colour he, therefore, rightly ascribed to the direct agency of light, rather than to that of heat or air†.

284. These experiments were carried much farther by M. Bonnet, who caused peas and beans to grow in glass tubes, when their colour was not affected; but in boxes, when the light was completely excluded, the plants became white. Even in wooden cases, however, the plants exhibited a green colour in particular parts, placed opposite to small perforations which were made to admit the light. The exclusion of light, he adds, obstructs the development of the leaves, and promotes the elongation of the stem; but neither want of air, nor a greater or less degree of heat, seemed to affect the degree of etiolation‡.

285. This philosopher farther discovered, that plants, which had become white by the exclusion of

* *Physiol. Veg.* vol. iv. p. 265.

† *Hist. Plantar.* vol. i. p. 15.

‡ *Recherches sur les Feuilles*, p. 210. 331.

light, recovered their green colour when the light was restored. Thus French beans, which were much etiolated, acquired, in 24 hours, a sensible green tint by exposure to day-light in summer; but, in the dark days of autumn, they still retained their whitish colour, even when thus exposed. He also noticed the great exertions which vegetables make to expose their leaves to the action of the sun; thus, a tuberous root, confined in a vessel where the light was partially excluded, made, in five weeks, eleven turnings or movements to follow it*.

286. The red and purple, as well as the green colours of vegetables, and all the varied hues of flowers, seem, likewise, to owe their perfection to the agency of light. Dr Grew remarked, that those parts of the roots of plants, which remain under the soil, are generally white, while the parts exposed above ground are frequently coloured; thus the tops of sorrel roots are red, and those of turnips and radishes are sometimes purple. These changes in the colours of the roots, as well as the green colour of the leaves, and the different colours of flowers, he attributed to the action of the air†; but the experiments of Mr Davy (25.) prove that the colours of flowers, as well as of leaves, depend immediately on the operation of light.

287. The changes produced in the other sensible properties of plants, by the agency of the solar rays, were first remarked by Scheele, who considered

* Recherches, &c. p. 340.

† Anat. of Plants, p. 270. 2d edit.

light to be decomposed in vegetables, and to assist in the formation of their resinous and inflammable matter *. In the year 1774, Dr Robison observed, that the leaves of tansey, which had grown in a coal mine, and were white, afforded no aromatic smell, when rubbed between the fingers; but after they were planted in a place exposed to light, their white leaves died down, and fresh green sprouts shot up, which possessed the smell and all the other properties of common tansey. He repeated the experiment with great care on lovage, on mint, and other plants. They all throve well in darkness, but with a blanched foliage, that had no resemblance to the ordinary foliage of the respective plants. When brought into day light, they all died down, and the stocks then produced the proper plants in their usual dress, and having all their distinguishing smells †.

288. After these general remarks on the agency of light in producing the colours, and otherwise changing the sensible properties of plants, we proceed to consider its power in modifying the qualities of the atmosphere, in which vegetables are made to grow. From the history of facts already given (243.), it appears that Dr Priestley first advanced the opinion, that plants, in certain circumstances, purified the air. This effect he considered to depend on the *vegetating state* of the plant; but as, on most occasions, he has omitted to state whether his experiments were conducted in sunshine, or in the shade, it cannot be said

* On Air and Fire, p. 158.

† Black's Chem. Lectures, vol. i. p. 532.

that he was, at all times, fully aware of the importance of the agency of light. In many instances, however, he has duly remarked it. Thus, for example, in his experiments on the green matter that forms on the sides of vessels filled with stagnant water, he asserts that the pure air is never produced in the shade, but only under a direct exposure to light; and no degree of warmth, he adds, will supply the place of the sun's rays. He farther observed, that the water, which contained most carbonic acid, afforded oxygen gas most abundantly; and that this gas was entirely dissipated by the action of the sun, leaving a residue of pure oxygen. When the glass vessels, containing this matter, were exposed to the sun, and were in the act of yielding pure air most abundantly, he found that the process ceased entirely, if he intercepted the solar rays by covering the vessels with black wax, or by removing them into a dark room *. These facts clearly demonstrate the power of light in the development of oxygen gas, and likewise furnish evidence of the decomposition of carbonic acid by the operation of the same agent.

289. The experiments of Priestley were prosecuted by his illustrious contemporary Scheele, who, as we have already seen, (255.) could never succeed in his attempts to purify noxious air by vegetation, whether his experiments were conducted under exposure to the sun, or in the shade. The cause of uncertainty in the experiments of Priestley, and of entire failure in those of Scheele, is ascribed, by Dr Ingenhousz, to

* Obs. on Air, vol. iv. p. 337. et seq.

the apparatus which they employed being placed indifferently in sunshine or in the shade ; or to their having suffered the plants to remain in confinement until they had lost their vigour or their life *. It must, however, be observed, in justice to those philosophers, that they were both duly apprized of these circumstances, and conducted many of their experiments with an immediate regard to them. While therefore we admit, that the variation in the results of some of Priestley's experiments may have arisen from inattention to these particulars, yet the discordance between him and Scheele is, more probably, to be ascribed to the different nature of the gases which they employed, in consequence of the precaution which the latter always adopted, of first removing the carbonic acid from the foul air which he submitted to experiment.

290. But certainly the experiments of M. Ingenhousz, both by their number and variety, have contributed, more than those of any other philosopher, to elucidate the subject of the purification of our atmosphere by plants. In air that had been so far depraved by respiration as to extinguish a lighted candle, he placed a plant of peppermint, and then exposed the vessel, for three hours, to the sun, at the end of which time the air again supported flame. When, however, the common nettle was placed in a similar portion of respired air during the night, the air was not at all improved ; but the apparatus being

* Exper. sur les Veget. tom. ii, pref. p. 31.

afterwards exposed to the sun for two hours, the air was brought back to its original purity. He also found, that mustard plants, when confined in a vessel of common air through the night, rendered it impure; but that this same air, together with the plants, being the next morning exposed to the sun, was restored, in an hour and a half, to its former purity*. When, likewise, similar portions of the same respired air were confined in vessels with similar plants, and respectively placed in sunshine and in the shade, the air, exposed to the sun, recovered its purity in a few hours, while that, placed in the shade, was rendered more impure than before. He repeated these comparative experiments a great many times, and varied them in different ways, until there remained not the least doubt, but that it was to the light of the sun that the purification of the air was owing, and to the absence of this light, that the opposite effect was produced†.

291. Dr Ingenhousz next made experiments on air that had been vitiated by the burning of a common candle, suffering the carbonic acid which it contained to remain in it. With this air he filled three bell glasses of the same size, and introduced into each glass some plants of young mustard. One of the glasses he exposed to the sun at mid-day; another he placed in the shade; and the third was removed to a place of perfect darkness. The next morning, before the sun had again shone on the

* Exper. tom. i. p. 263. 265.

† Exper. &c. tom. ii. pref. p. 31.

plants, he examined the air of the first glass, and found it to be considerably vitiated, and to contain a little carbonic acid, which he ascribed to the nocturnal effect of vegetation; while the air, in the two other glasses, contained one-fifth of acid, and was greatly more vitiated. Similar experiments were made on three different portions of common air, and with nearly the same results *. In the shade, he adds, plants deteriorate the air as much as in perfect darkness, although the colour, which they assume in darkness, differs much from that which they acquire in the shade; for, in the latter, they retain their verdure, while, in the former, they become white or yellow †.

292. Besides these experiments on the power possessed by plants of purifying the air vitiated by the several processes of respiration, vegetation, and combustion, Dr Ingenhousz made various others with the leaves of plants immersed in water, and exposed to the light of the sun. From these experiments, he was led to conclude, that plants not only correct impure air in the space of six days or more, as Priestley's experiments seemed to indicate, but that they perform this important office in a very few hours; that this operation is not owing to *vegetation*, but to the influence of the solar light on plants; that plants possess the power of giving out, in a pure state, the air which they contain and have derived from the atmosphere; that this operation commences some

* Exper. t. ii. p. 45. 46.

† Ibid. p. 47.

time only after the sun has gotten above the horizon, is more or less vigorous in proportion to the clearness of the day, and the situation of the plant to receive the direct rays of the sun, and is suspended entirely during the darkness of the night; that plants, in the shade, do not ameliorate the air, but render it more noxious; that the leaves, and green branches and stems of plants alone perform this office of purification; and that acrid, offensive, and poisonous plants act in the same manner as the most sweet and salutary; that the pure air proceeds chiefly from the inferior surface of the leaves, and that young leaves do not furnish so much of it as those which have acquired their full size and vigour; that some plants yield purer air than others, and that aquatic plants particularly excel in this respect; and, lastly, that the sun does not possess the power of improving the qualities of atmospheric air without the concurrence of plants. These conclusions were drawn from the results of more than 500 experiments, made between the months of June and September *.

293. Of this great number of experiments, however, almost all were made with the leaves after they were separated from the branches, and immersed in glass vessels of water. Dr Ingenhousz says, that this mutilation does not vary the result; for when he passed the pliant branches of vegetables, whose roots remained in the earth, into jars of water, the same phenomena were exhibited; and therefore, he afterwards used the leaves and cut branches as being most convenient †. And with respect to immersion in wa-

* Exper. t. i. pref. p. 64. et seq. † Ibid. p. 283.

ter, he does not consider it so foreign to the nature of vegetables as to derange their functions. Many plants, he observes, will vegetate a long time, although covered entirely with water, and aquatic plants live continually in it. In fact, the water, according to him, only intercepts the communication between the plant and the atmosphere; and though it prevents the plant from receiving any thing from the air, it does not put any obstacle to the discharge of what it contains *.

294. These ideas may, to a certain extent, be correct. M. Bonnet has shewn that the leaves of various plants, when separated from their branches, continue fresh several weeks, and, in some instances, even months, when their surfaces are laid on water. In some cases, the experiment succeeded best when the upper surface of the leaf, and in others, when the under surface, was applied to the water †. It may be doubted, however, whether leaves continue fresh so long when they are completely immersed, since the means of evaporation are then cut off, and consequently the water received must remain stagnant in the leaf. In the greater number of Dr Ingenhousz's experiments, however, the process continued only a few hours, and the leaves, therefore, though unable to vegetate, may be still considered as possessing life.

295. The employment of water is farther objectionable, because it not only removes the vegetable from

* Exper. t. i. sec. 8. p. 43.

† Recherches sur l'Usage des Feuilles, p. 8.

its natural situation, but places it in circumstances where the external air is entirely excluded. The existence of aquatic plants under water is no proof that terrestrial ones are able to support a condition so unnatural to them ; for the structure in each may be very different which enables them to live and flourish in situations so very dissimilar. Thus we know that the leaves of the *ranunculus aquaticus*, which float on the surface of the water, have a flat and expanded form, while those, which are entirely immersed, exhibit a roundish and filamentous figure.

296. But it is another objection to these experiments, that they bring many new sources of fallacy into view, which greatly embarrass the original question ; since it is not easy to determine how much of the air is furnished by the water itself, and how much by the vegetable immersed in it. These, accordingly, have been points much disputed among those who have made experiments on the subject. We prefer, therefore, those experiments of Dr Ingenhousz which we have before detailed (290. 1.), as affording the best evidence adduced by him of the purification of the atmosphere, by plants exposed to the sun.

297. In these experiments, the air, vitiated by respiration and combustion, appears always to have contained carbonic acid, at least no notice is given of this acid having been previously removed. Farther, Dr Ingenhousz supposed plants to possess the singular property of converting carbonic acid into respirable air, when exposed to the sun ; and he asserts, that if they are confined in mixtures of common air

and carbonic acid, and exposed to the solar influence, this acid gas will be changed into an air insoluble in water *. Lastly, we may add, that the rapid manner in which the purification of the air is stated to have occurred in the experiments of Dr Ingenhousz, effectually excludes the idea of the oxygen gas being derived from decomposition of the vegetable substance.

298. M. Senebier instituted, likewise, a great number of experiments to prove the production of oxygen gas by plants, exposed to the direct rays of the sun. Almost all of them were, however, made by immersing leaves in water, previously impregnated with carbonic acid; and they do not apply, therefore, directly to the present question. In one instance, indeed, he placed a branch of raspberry tree, having its end dipped in water, in a recipient filled with common air, and inverted in water. The recipient was then exposed, through the day, to the direct influence of the sun. The air at first, when examined by nitrous gas, was reduced to 1.04, and at the close of the experiment, when again tried, it was reduced to 1.00; so that, if the method of analysis be deemed correct, it had experienced some improvement.

299. M. Senebier considers the agency of the sun to be indispensable to the production of the oxygen gas which leaves afford, when confined either in water or in air; and this operation of light, he adds, is continued through the winter, by those plants which retain their verdure. He has seen mosses, when pla-

* Exper. t. ii. p. 74. 77.

ced under water and exposed to the sun, afford pure air, when the temperature was many degrees below zero ; and many evergreens, under similar circumstances, are always able to afford it. In every case, he believes the oxygen to be derived from the decomposition of carbonic acid, which he supposes to be effected in the leaves by the affinity of light for oxygen ; and the oxygen produced is, he adds, always proportional to the quantity of carbonic acid that is present. He denies that oxygen is ever derived from the decomposition of water ; for in boiled water, which contains no carbonic acid, plants do not afford oxygen, while in waters impregnated with this acid gas, they yield it most abundantly. Neither is the oxygen furnished solely by the leaves, independently of light, since it is not obtained from them when the apparatus is placed in the shade *. From these facts we collect, that the presence of carbonic acid is necessary to the production of oxygen, which can be furnished only by the decomposition of this acid gas through the joint agency of the plant and of solar light.

300. In 1779, Mr Davy made some experiments with the view of discovering the origin of the oxygen gas which vegetables, in certain circumstances, seem capable of affording. He introduced a small plant of *arenaria tenuifolia*, growing in a pot of moist earth, into a jar containing 14 cubic inches of carbonic acid. The jar was inverted in mercury, and exposed to the solar rays, for four successive

* Phys. Veg. t. iii. p. 195. et seq.

days of fine weather in the month of July. The plant, at this period, looked well and healthy; the mercury had risen considerably into the jar; and on examining the gas, there was a deficiency of 2.3 of an inch. After removing all the carbonic acid by an alkaline solution, the remaining gas measured two cubic inches, and proved to be oxygen. This experiment he repeated two or three times on different plants with similar results.

301. That the oxygen, obtained in these experiments, did not proceed directly from the vegetable, Mr Davy ascertained, by having, at the same time, kept another plant, in all circumstances exactly similar, in a jar of nitrogen gas. The diminution of bulk in this gas did not exceed $\frac{3}{8}$ of an inch; and $\frac{6}{10}$ more were abstracted by the hydro-sulphuret of potassa, which he considered to be oxygen formed from the decomposition of the water of the plant*.

302. Mr Davy having thus ascertained the fact of the decomposition of carbonic acid, next made experiments to prove that plants, by their own powers, independently of the agency of light, were unable to effect this decomposition. He placed a small plant of *chironæa centaurium* in a jar of pure carbonic acid inverted in mercury, and then set it aside in a dark closet. At the end of four days, the air had diminished a little, and the plant looked sickly. The air was now examined; and after the carbonic acid was removed, there remained only a small portion of an incombustible gas.

* Beddoes' Contributions to Science, p. 163.

303. As in the preceding experiment, the plant was unable to decompose carbonic acid without the aid of light, so likewise he found that light, by itself, was alike insufficient for this purpose; for no alteration was effected in this acid gas, though exposed, for any length of time, to the action of the solar rays *. It follows from the foregoing series of facts, that neither the power of the plant alone, nor the agency of light alone, is able to decompose carbonic acid, but that its decomposition is effected by the power of the plant, in concurrence with the direct agency of the solar rays.

304. Dr Woodhouse exposed the leaves of nine different plants in separate vessels of atmospheric air, mixed with about $\frac{1}{10}$ of carbonic acid, to the light of the sun for seven hours; at the end of which time, he found that the acid gas had disappeared, and the atmospheric air was greatly increased in purity. When the air was previously vitiated by carbonic acid, formed by the putrefaction of a fungus, he found it to be restored to a great degree of purity by fresh leaves exposed to the sun; but when similar leaves were kept in air through the night, carbonic acid was always produced, and the purity of the air thereby diminished. He maintains, as Senebier had before done, that oxygen gas is never produced by plants, unless carbonic acid previously exist in the air; and he ascribes the production of oxygen gas, in Priestley's experiments on vitiated air, to the pre-existence of carbonic acid in it †.

* Contributions, p. 161.

† Nicholson's Journal, 8vo, vol. ii. p. 153, & seq.

305. The last author who has directed his attention to the effects produced in the air by living plants, exposed to the direct influence of the solar rays, is M. T. de Saussure, in his elaborate treatise on vegetation, to which we have already so frequently referred. In many examples, however, atmospheric air does not seem to have experienced any amelioration; which accords with the observation of Senebier and Woodhouse, that plants do not, even in sunshine, sensibly improve the atmosphere, unless it previously contain carbonic acid. M. de Saussure found, that three young peas, about 4 inches high, placed in 50 cubic inches of pure atmospheric air, and exposed, from five to six hours a day, to the direct rays of the sun for ten days, in which time they increased in weight eight grains each, had not sensibly changed either the purity or volume of their atmosphere*. When the atmospheric air was previously washed in lime water, to free it from the carbonic acid naturally present in it, the same results were obtained. After five or six days direct exposure to the sun, the air remained unchanged, both in purity and in volume, though the plants continued sound and vigorous in all their parts†.

306. Similar results were afforded by plants of *vinea minor*, placed in 290 cubic inches of atmospheric air, and exposed, six hours a day, for six days, to the light of the sun; at the end of the experiment, the plants were found to have suffered no alteration, nor to have produced any change either in the purity

* Recherches, p. 29. 32. † Ibid. p. 34. 35.

or the volume of the air. In the same manner, the atmospheric air, in which two plants of *mentha aquatica* had grown for ten days, and been exposed six hours each day to the rays of the sun, experienced no change either in its purity or its volume. The *lythrum salicaria*, vegetating for seven days, under a similar exposure to the sun's rays, produced no change in the volume or composition of its atmosphere; and M. de Saussure adds, that he could not be mistaken in the measurement of the volume of air more than one-fourth of a cubic inch. The *pinus genevensis* and *cactus opuntia*, placed in the same circumstances, were likewise found to produce no sensible change in the purity or volume of their atmosphere *. Hence we learn, that, when plants are confined in given portions of air, and placed alternately, for several days, in sunshine and in the shade, they neither vitiate nor improve the atmosphere in which they are made to grow.

307. But we have before seen, that, in the shade, plants, by their growth, uniformly convert the oxygen gas of the air into carbonic acid, and such conversion, we have argued, is the natural and necessary effect of vegetation. If, therefore, no acid be found in the air, when the process takes place in sunshine, either that gas cannot, under such circumstances, have been formed in vegetation, or, if it has been formed, it must at once have been reconverted into oxygen gas by the agency of the solar rays. Now since, in these experiments, the plants actually grew; since oxygen gas is necessary (30.) to vegetation, and

* Recherches, p. 42. et seq.

is, by that process, converted into carbonic acid, such conversion must have occurred while these plants were kept in the shade ; consequently, although no carbonic acid was found in the vessels after they had been exposed to the sun, its non-appearance is not to be received as proof against its formation in the shade, but only of its reconversion into oxygen gas under exposure to the sun.

308. This inference is completely borne out by the results of experiments related by M. de Saussure. He confined plants in vessels of atmospheric air, from the top of which he had previously suspended a portion of lime ; and then placed them to vegetate in sunshine. On the second day, the atmosphere was diminished in volume ; and on the fifth day, the air was examined and found to be much depraved : it contained only $\frac{16}{100}$ of oxygen gas, and no sensible quantity of carbonic acid. This experiment he repeated many times with different plants, and always with similar results. We see, by these experiments, says M. de Saussure, that there had been an attraction, and consequently a formation, of carbonic acid ; and we observe also another remarkable fact, namely, that plants, even in sunshine, produce, with the oxygen of the atmosphere, pure carbonic acid. It is, therefore, only because they decompose it, after having formed it, that they do not, like animals, vitiate the atmospheric air in which they are made to grow*. Hence then we learn, that, in sunshine as well as in the shade, plants, by their growth, naturally convert the oxygen gas of the air into carbonic acid.

* Annales de Chimie, tom. xxiv. p. 145.

309. That vegetables, however, with the aid of the solar rays, also possess the power of decomposing this acid, and reconverting it into oxygen gas, seems sufficiently established by the numerous facts already stated: and various experiments, related by M. de Saussure, abundantly confirm the same position. He found, as Drs Priestley and Percival had long before done, that plants would not, even in sunshine, vegetate in pure carbonic acid. If their atmosphere contained two-thirds or three-fourths of this gas, it was alike fatal to them. They vegetated seven days in an atmosphere containing one half its volume of carbonic acid; ten days in one which contained one-fourth; and when it contained one-eighth, they have acquired nearly as great a weight as in atmospheric air. Lastly, when only one-twelfth of carbonic acid was present, the plants, he says, have increased in weight, and prospered better than in pure atmospheric air*. In the shade, the smallest quantity of carbonic acid, added to common air, is injurious to vegetation. The plants have died in six days, when it constituted one-fourth of their atmosphere; and even when it was only one-twelfth, their vegetation has been feeble, and their increase of weight small. In all cases, oxygen gas, he adds, must be present; for if a small portion of carbonic acid be added to pure nitrogen gas, plants die in it, even though exposed to the sun†.

310. Having thus ascertained the general effects of carbonic acid on vegetation, he proceeded to make

* Recherches, p. 31.

† Ibid. p. 33.

direct experiments on its operation, when introduced in small quantity, and exposed to the influence of the solar rays. He procured seven plants of *vinea minor*, each about eight inches high, and placed them in an artificial atmosphere of 290 cubic inches, composed of common air, mixed with $\frac{7\frac{1}{2}}{100}$ of carbonic acid. The roots of the plants were immersed in a little water, and the vessel that contained them was inverted in mercury, covered by a thin film of water, to prevent its noxious operation on the plants*. This apparatus was exposed six hours to the direct rays of the sun, for six days. On the seventh day, the plants were withdrawn, and had undergone no alteration: the bulk of air had not apparently increased nor diminished: neither did it exhibit any sign of containing carbonic acid, when washed with lime water. To make up, however, for the 21.75

* M. de Saussure observes, that this effect of mercury on vegetables was first noticed by the Dutch chemists, and he found it always to operate when the experiment was continued for any length of time. In one instance, where we had confined some willow slips standing in the shade, in a small bottle of water under a jar of atmospheric air, inverted over mercury, the leaves, in two days, were changed to a dark brown colour, and by the third day were quite black. This blackness extended through their substance, and they easily separated from the stem, though they had no putrid smell. The air did not contain any sensible quantity of carbonic acid, and afforded nearly its full proportion of oxygen. It was not in the least altered in bulk, for only a small globe of mercury, not bigger than a pin's head, had gotten within the jar, and the lowest part of the leaves was more than three inches above it, which was the height of the phial, in which the vegetables were placed.

cubic inches of carbonic acid originally present, but which had now entirely disappeared, the atmosphere was increased by 14.75 cubic inches of oxygen gas, and seven cubic inches of nitrogen, which, together, supply exactly the bulk of the carbonic acid lost *. Similar experiments were made with plants of *mentha aquatica*, *lythrum salicaria*, *pinus genevensis*, and *cactus opuntia*, and nearly with the same results ; the carbonic acid having, in all cases, more or less completely disappeared, and its place being supplied by nearly an equal quantity of oxygen, with a small portion of nitrogen gas †.

311. The foregoing experiments seem to afford undeniable evidence of the decomposition of carbonic acid by growing vegetables, when they are exposed to the direct rays of the sun, since not only did this acid uniformly disappear, in circumstances where no substance was present, which could attract or combine with any considerable portion of it, but a large volume of oxygen gas was produced, without the vegetables having undergone any change, by which they can have been rendered able to supply it. That this superabundant oxygen did not proceed from the decomposition of the plant, or of the water which it contained, is farther proved by the results of the experiments (305.), made in pure atmospheric air. These experiments were purposely made at the same time, with similar plants, and under the same circumstances ; and yet, as we have seen, no absolute

* Recherches, p. 41.

† Ibid, p. 44. & seq.

addition of oxygen gas was made to the atmosphere in which the vegetables grew, but both its volume and composition remained permanently the same. Even if decomposition of the vegetables had taken place, we do not know that they could have directly afforded pure oxygen gas; but we do know that this gas is furnished by them in union with carbon; so that the existence of an additional quantity of oxygen, in such circumstances, would have afforded equal evidence of the decomposition of carbonic acid, by the agency of solar light.

312. But not only by the direct rays of the sun, but without the intervention of a strong light, it is probable, says M. de Saussure, that plants, growing in atmospheric air, decompose a part of the carbonic acid, which they themselves form out of the surrounding oxygen gas, although the fact be not susceptible of direct proof. Marsh plants, as the *polygonum persicaria* and the *lythrum salicaria*, yielded oxygen gas by a weak and diffused light, when confined in an atmosphere of nitrogen*: and different species of *epilobium* vegetated a long time, and grew as well in pure nitrogen gas, as in common air, though exposed only to a weak light, or protected from the direct action of the sun. The nitrogenous atmosphere, at the end of two months, was increased in bulk, and contained $\frac{5}{100}$ of oxygen gas. When similar plants were confined in pure nitrogen gas, and kept in perfect darkness, though they were renewed

* Recherches, p. 54.

every twelve hours, lest their vegetation might languish, yet they produced no oxygen gas, but augmented their atmosphere, by a quantity of carbonic acid*.

313. Even in perfect darkness, continues M. de Saussure, some plants would seem to possess the power of decomposing carbonic acid. He confined plants of peas and of salicaria, in two equal recipients of atmospheric air, and set them aside in a place perfectly dark. One of the recipients contained a portion of lime or potassa, while the other held the plants only: in both vessels, the plants were daily renewed. At the end of four or five days, the air, in both recipients, was vitiated; and in several repetitions of the experiments, he constantly found that the air of the vessel, which held the lime or potassa, contained less oxygen gas, than that of the vessel which had no alkali. This difference, it is added, may be presumed to arise from less acid being present for decomposition in the vessels, which contained the alkali, than existed in the recipients which held no alkaline substance†.

314. We have already stated (25.) that artificial light changes the colour of plants in a manner similar to that of the sun. M. Decandolle found, that various plants, raised from seeds in a vault lighted by lamps, became green both in their stems and leaves, nearly like those which grew in the open air. He confined also the leaves of various plants in inverted glass vessels of water, and placed them in a

* Recherches, p. 199. 201.

† Ibid. p. 55.

vault illuminated by six lamps, which afforded light equal to fifty ordinary candles. For the first twenty-four hours, no air was afforded, but at the end of that time it collected in considerable quantity ; and, on being analysed by M. Vauquelin, was found to consist principally of nitrogen and carbonic acid, with only $\frac{2}{108}$ of oxygen gas *. These experiments shew, that though artificial light renders vegetables green, yet it is not sufficiently powerful to decompose carbonic acid in plants, at least in any considerable quantity ; and in this respect, therefore, it resembles, in its operation, the effects of a weak natural light.

315. The probability of the decomposition of carbonic acid by plants, growing in a weak light, is supported by many facts, which seem otherwise incapable of explanation. The germination of seeds we have seen to produce a considerable consumption of oxygen ; and it might, therefore, be naturally expected that plants should continue to affect the air at least in an equal degree. The changes, however, effected by plants in a state of full growth and vigour, on the surrounding air, says Mr Murray, are much less than have been imagined, and, when supplied merely with water, they are so inconsiderable as not to be very perceptible. Hassenfratz, he continues, inclosed growing plants in atmospheric air for a month and a half or two months at a time ; and though the plants grew, the air remained unaltered in volume, and in the proportion of oxygen †. This slowness of change

* Journ. de Phys. tom. lii. p. 124.

† Syst. Chem. v. iv. p. 54.

in the air is remarked, likewise, by Dr Woodhouse, who says that a plant in perfect health, confined in atmospheric air, will live in it for a long time without producing any sensible change. In his experiments, many vegetables did not affect the air in five days; some diminished its purity in three hours; and others altered it in a most slow and gradual manner, causing little change in it in twenty days*.

316. If, therefore, we compare this slow consumption of oxygen gas by plants, with its more rapid conversion by seeds, we must either suppose that plants require the aid of those advantages, derived from the air, in a degree infinitely less than seeds; or, what is more probable, we must believe that the necessary changes, which their vegetation produces in the air, are, at all times, more or less counteracted by some other office which they perform; in other words, that the oxygen gas, which is converted into carbonic acid by the act of vegetation, is again restored, more or less completely, to the atmosphere, by some other process. This restoration we have seen to be actually made, and it now only remains that we endeavour to discover the manner in which it is carried on, and try to ascertain whether it be, or be not, executed by the proper vegetative powers of the plant.

* Nicholson's Journal, vol. ii. p. 152.

317. If we take a retrospective view of the circumstances in which oxygen gas was produced in all the experiments which have been now detailed, it will, we think, appear, that, conformably to the opinion of Senebier and Woodhouse, carbonic acid gas was always previously present. Thus, when Dr Priestley employed air that had been vitiated by respiration, combustion or putrefaction, which for the most part contains carbonic acid, the air was frequently much ameliorated and rendered capable of again supporting vegetable life; but when he withdrew the oxygen gas of the atmosphere, by means which imparted to it no carbonic acid (247.), the plants never failed to die. So, likewise, M. Scheele was never able to render vitiated air pure, even by exposure to the sun, because he previously abstracted (247.) from it the carbonic acid which it contained. In the air, depraved by respiration and combustion, which Ingenhousz employed (290. 1.), carbonic acid seems always to have previously existed. The experiments and observations of Senebier (298. 9.) and Woodhouse (304.) altogether tend to prove, that oxygen gas is never afforded by the leaves of plants, either in water or in air, unless carbonic acid be present; and that the quantity of oxygen supplied is in proportion to the carbonic acid that disappears. Lastly, in the experiments of De Saussure we have seen, that, when no carbonic acid previously existed in atmospheric air (305.), no additional quantity of

oxygen was afforded to it; but that, when this acid gas was first mixed in certain proportions with the air, it constantly disappeared (310.), and the atmosphere then contained a superabundant quantity of oxygen gas. The numerous and varied proofs of the decomposition of carbonic acid, afforded by the experiments of Priestley, Ingenhousz, Senebier, Davy, Woodhouse, and De Saussure, add great weight to the opinion for which we now contend, since they amount to demonstrative evidence, that carbonic acid is actually decomposed by the concurring operation of the green parts of vegetables and of solar light.

318. In what manner, then, does carbonic acid obtain admission into the vegetable, so as to be afterwards afforded by it in the form of oxygen gas? It is well known, that this acid is readily attracted by water, and is almost universally contained in it. Water, it is likewise well known, is absorbed by the roots and leaves of plants; and that it carries air with it into the vegetable body, is established by numerous observations. Dr Hales remarked air bubbles to rise, very abundantly, in hot weather, from vegetable sap collected in tubes, so as to make a froth an inch deep at the top; and when, also, the sap was placed in an air pump, it afforded, as the receiver was exhausted, great plenty of air bubbles*. Mr Knight found the sap, whether extracted from the tree near to the ground, or at a distance from it, al-

* Veg. Staties, vol. i. p. 125. 3d edit.

ways to contain a large portion of air * ; and M. Coulomb, on piercing an Italian poplar, observed the sap to flow out abundantly, with a continued noise of air bubbles, which ascended and burst in the orifice †. These bubbles M. Senebier found, in some cases at least, to be carbonic acid ; for if the clear sap of the vine was mixed with lime water, a white precipitate was formed, which could again be dissipated with effervescence by a few drops of nitric acid ‡. M. Vauquelin also, in his analysis of the sap of the elm, found it to contain lime, which was held in solution by carbonic acid, of which there existed a considerable excess in the sap ||. These facts, therefore, afford sufficient evidence of the existence of this acid gas in the sap of plants, and of its escape when this sap comes into contact with the air.

319. Besides the carbonic acid, which thus enters the substance of the plant by the medium of water, other gases, dissolved in water, in the same way gain admission into the vegetable body. Thus, the experiments of De Saussure seem clearly to prove, that oxygen gas may, in this manner, be carried into the vegetable ¶ ; and since nitrogen gas is likewise contained, in a smaller proportion, in water, it may be presumed, that, by a similar conveyance, it gains admission also, and is again given out unaltered in those

* Phil. Transact. 1805.

† Phil. Magaz. vol. ix. p. 310.

‡ Physiol. Veg. tom. ii. p. 343.

|| Thomson's Chem. vol. iv. p. 262. 1st edit.

¶ Recherches Chim. p. 111.

cases (310.) where we have seen it to be afforded by plants in confined air.

320. The gases, which thus enter by the roots and exist in the sap, will be conveyed with it to the leaves. M. Senebier plunged branches of the peach tree into recipients containing water charged with carbonic acid; so that, in some instances, the ligneous extremity of the branch was also immersed, while, in other instances, this extremity was confined in an empty bottle. The result was, that the leaves of the branches, whose ends were immersed, afforded in sunshine double the quantity of oxygen that was yielded by the others; whence he concludes, that the carbonic acid passes with the water into the leaves and is then decomposed *. In the experiments of M. de Saussure, the attraction of oxygen by water, the absorption of this water by the roots, and the subsequent expulsion of oxygen from the leaves, are clearly established †; so that the conveyance of gaseous fluids into the leaves, by this route, cannot be reasonably doubted.

321. But not only by the roots does carbonic acid thus obtain admission into vegetables, but likewise by the leaves. The power of the leaves to absorb water has been amply established by the experiments of Bonnet; and the gaseous fluids, which exist in this water, may readily be conceived to enter with it. The production of oxygen gas by leaves, immersed in water containing carbonic acid (288.), can-

* *Physiol. Veg.* tom. iii. p. 225.

† *Recherches*, p. 111.

not be explained without supposing this acid first to enter, in solution, into the substance of the leaf; and that it actually does enter, seems to follow from the experiments of M. Senebier. He immersed the leaves of *sedum* in boiled water, and placing them under the receiver of an air pump, exhausted the vessel till the leaves sank. They were then removed into water impregnated with carbonic acid, and, in seven minutes, they rose from the bottom of the vessel, and, when exposed to the sun, afforded oxygen gas in great abundance; which proves, he adds, that leaves imbibe this acid gas, and again expel it in another state, under exposure to the sun. When, on the contrary, the leaves were thoroughly exhausted of air, and exposed to the sun in boiled water, in which no air existed, they did not furnish an atom of air *.

322. Much attention has been employed to ascertain the part of the leaf in which the carbonic acid is contained and decomposed, and the surface from which it afterwards issues. As the gas itself is invisible, its escape can be detected only by placing the leaves in water; or, if they are retained in air, by marking the increase of volume which the air may receive. This latter method, however, enables us to judge only of the fact, but not of the mode in which it is accomplished. From experiments, made by immersing leaves in water, Dr Ingenhousz supposed the air to issue chiefly from the *inferior* surface of the leaves. From the leaves of some fleshy plants,

* Phys. Veget. tom. iii. p. 242.

which were cut in pieces and exposed in water to the sun, he observed very pure air to issue, in continual jets, from their parenchymatous structure; but in other plants, which afforded air very abundantly, its escape was not visible from either surface of the leaves *.

323. M. Senebier also found the *inferior* surface of the leaves to yield the most air; that the epidermis, when removed from the other parts, afforded no air, but that the air then issued abundantly from the denuded parenchyme; wherefore he concludes, that the parenchyme is the true seat of the air that is obtained. The air, which thus exists in the parenchyme, he supposed to escape through the epidermis; for when the surfaces of the leaves were covered with paste or varnish, no air then issued; but he was never able to discover the pores or orifices through which it obtained a passage †. The foregoing observations and experiments seem to prove that the gaseous fluids of plants exist principally in the parenchyme of the vegetable, and from thence issue into the surrounding atmosphere.

324. According to M. Jurine, the parenchyme of the leaf is composed of an aggregation of small cells or *utriculi*, in which a green juice is contained. The form of these cells is either spherical, cylindrical, elongated or irregular; and according to their form, the spaces between them, which he calls *utricular interstices*, will be more or less large. It is in these

* Exper. tom. i. p. 12. 24. 26.

† Mem. Physico-Chimiques, tom i. p. 128. et seq.

interstices that the air, which leaves contain, seems to be lodged. In a leaf of fritillary (*fritillaria*) and of purslane (*portulaca*), small luminous points may be seen by the microscope, or even by the naked eye, which points are said to be produced by the light reflected from the air contained in the utricular interstices. To prove this, he removed the pellicle of a leaf of fritillary, and cut the parenchyme, so that he could observe several of these luminous points; and then, by gently compressing the parenchyme under water to force out the air, he saw it issue in the form of bubbles, and the water, having taken its place, rendered the interstices transparent, which were before opaque. In the petal of the rose, these inter-cellular spaces are very large; and if the petal be slightly compressed under water, the air it contains will be seen to move with rapidity, following the different inflections of the utricular interstices*.

325. But for the escape of the air thus lodged in the interstices of the parenchyme, some passages must exist, and these are to be found in the external pores. It has been much disputed among late writers, whether the leaves of plants are furnished with a proper and distinct *epidermis*. The late M. de Saussure considered what is called the epidermis to be composed of three different textures. M. Mirbel, however, denies its existence, and considers, what is so called, to be the union of the contiguous surfaces of the external cells; and M. Jurine, also, describes this covering as formed by the exterior stra-

* Philos. Mag. vol. xvi. p. 109.

tum or face of the *utriculi*, for he could never find the distinct epidermis of Saussure. But whether this covering be, or be not, a distinct membrane, it is admitted by all to be furnished with innumerable pores. In a leaf of the bulbous lily, Hedwig counted 577 pores in the small space of a square line; and M. Jurine reckoned 140 in the same space of a leaf of fritillary. These pores are in form either oblong, oval, or irregular; and they extend, in different directions, over the surface of the leaf. Some leaves have them on both sides; some, on one side only; and other leaves are wholly destitute of pores. In the leaves of trees, they are chiefly distributed on the under surface, in the juniper on the upper, and in the fir and larch they are found on both surfaces. All the parts of the flower are likewise furnished with them. When observed by the microscope, after moistening the pellicles, the pores appeared to M. Jurine to be filled with a black matter, which was a small bubble of air contained in them; for, by gentle pressure with a fine needle, the bubble was detached, and the part then became transparent. And when the pores of different pellicles, previously removed from the leaves, were compressed under water, the air that issued from them gradually disappeared, which led him to conclude that it was carbonic acid.

326. That these pores communicate with the utricular interstices before described, he supposed from the following experiment. He placed the leaves of the *geranium peltatum* and *rumex sanguineus* in water under the receiver of an air pump, and observed air

to appear on both their surfaces when the receiver was exhausted, and again to disappear, when the equilibrium was restored ; from which he concluded that the air had been first drawn out of the leaf, and had afterwards re-entered it. In the *olea fragrans*, the upper surface of whose leaves is destitute of pores, the inferior surface alone was covered with bubbles, which disappeared in the same manner when the atmospheric equilibrium was restored *. It must however be observed, that the air, in these experiments, might be derived from the water, and its disappearance be caused by its re-attraction by that fluid, when the pressure of the atmosphere was restored. That air, however, does escape from leaves *in vacuo*, is rendered certain by the experiments of M. Senebier, who found a leaf of *sedum* to sink in boiled water placed under an exhausted receiver, and again to swim after being placed for a few minutes in water charged with carbonic acid †.

327. Besides entering vegetables through the medium of solution in water, air seems to gain admission into them by other means. According to Dr Bell, Dr Hill first shewed the cuticle of vegetables to be an organised vascular substance, which, in trees and shrubs, has external openings, but not in herbaceous plants. When a portion of a tree is placed under an exhausted receiver, air enters through the cuticle and issues from the wood, and we may therefore conclude that the proper entrance

* Philosophical Magazine, v. xvi. p. 4. 11. 110.

† Physiol. Veget. v. iii. p. 242.

of air into plants is through these cuticular vessels *. Many experiments of a similar nature were made by Dr Hales, who concluded from them that air entered plants not only with the nutrient matter by the roots, but also through the bark and the leaves †. These experiments, however, as Dr Smith observes, prove only that the vegetable body is pervious to air when emptied of its sap, or when pressed by the weight of the atmosphere ‡; but they afford no proof that the transmission of air through the vegetable structure is a natural and necessary function of vegetation.

328. Some experiments of Dr Priestley furnish evidence of a different nature respecting the entrance of air into plants. He found that the willow plant (*epilobium hirsutum*), while growing in water and confined in jars of hydrogen gas, greatly diminished the bulk of gas, and rendered it more pure; so that it was affected by nitrous gas, and exploded, when fired, like a mixture of common and inflammable air. The leaves continued green, and appeared to be always loaded with air bubbles, which were continually detached, and their place supplied by others. These bubbles, he supposed to be oxygen gas, and to have been the cause of the purification of the hydrogenous atmosphere; and he believed them to proceed from the plant itself, and not to be separated from the water in which it grew §.

* Manch. Mem. v. ii. p. 403.

† Veg. Statics, vol. ii.

‡ Introduct. to Bot. p. 200.

§ Obs. on Air, vol. v, p. 9, 10.

329. According to Dr Ingenhousz, plants, inclosed *in vacuo*, continually give out air, which in sunshine is oxygen gas, and in the shade is carbonic acid and nitrogen. The air contained in onions and in many aquatic plants he represents to be nearly the same as that which surrounds them. In tubes filled with nitrogen, the gas afforded by rushes was of the same quality; and when the tubes were filled with oxygen, the plants afforded gas of the same degree of purity. Similar results were obtained when the tubes were filled with hydrogen or carbonic acid gases, the air afforded being in every case similar to that which previously surrounded the plants. The experiments succeeded in sunshine and in the shade, and the change in the quality of the air was effected in half an hour; so that it is concluded, that the air existing in plants is never stagnant, but is constantly and speedily renewed*.

330. M. Senebier ascertained, by experiment, the truth of the foregoing observations, as far as they relate to plants which have a reservoir for air, such as the stems of onions and rushes; but that they do not hold with regard to plants of a different structure. The air which generally issued first from plants *in vacuo* he found to be nearly similar to that of the atmosphere, but it diminished in purity as the experiment was prolonged. It varied, however, in the same plants, at different times; and contained different proportions of oxygen, carbonic acid, and nitrogen gases†.

* Exper. t. ii. p. 96. 99.

† Physiol. Veg. t. iii. p. 120. et seq.

331. Many experiments of M. de Saussure seem, likewise, to prove that air exists naturally in plants. He found that green-leaved plants, especially such as had fleshy leaves, lived a long time in pure nitrogen, and afforded to it a considerable portion of oxygen gas in the sun, but yielded only carbonic acid in the shade. Similar results were obtained when the plants were confined in an atmosphere of the gaseous oxide of carbon. Many fleshy plants continued, also, to vegetate *in vacuo*, which M. de Saussure supposed them capable of doing only in consequence of the air they contained in their parenchymatous structure, which air was decomposed and eliminated by the action of the sun *. If, farther, we believe that the decomposition of carbonic acid by leaves is effected only in their parenchymatous structure, we must necessarily suppose, that, in the experiments of Davy and De Saussure, as well as in all others in which this gas was decomposed, it must first have entered into the substance of the leaf. From the facts, therefore, which have now been stated, it seems to follow that gaseous fluids obtain admission into plants, both through the medium of solution in water, and under an elastic form.

332. But the carbonic acid, which may thus enter vegetables, we have seen to undergo decomposition (317.), and to be partly expelled in the state of oxygen gas. How far, then, are we entitled to consider these operations as the necessary result of a living vegetable function? Preparatory to the decision of this question,

* Recherches, p. 200. 9. 14.

it may be useful to bring together and contrast the peculiarities which distinguish, respectively, the consumption and the production of oxygen gas by living plants ; for whether we regard the agents employed, the mode and circumstances in which they act, or the results which are afforded, there are no two processes, which, in many respects, present phenomena more dissimilar. Both in the shade and in sunshine, however, oxygen gas is essential to vegetation ; but in one situation, it is converted into carbonic acid ; in the other, this acid is re-converted into oxygen gas. Without the presence of oxygen, the living plant is unable to survive ; but an atmosphere of carbonic acid speedily destroys its life. When oxygen is consumed, the union of that gas with carbon appears to take place exterior to the vascular system of the plant ; when the same gas is produced, the decomposition of carbonic acid is effected in the cellular structure of the leaf. To the continuance of living action the formation of carbonic acid seems to be essential ; but its decomposition is, in no respect, necessary to the life of the plant. The one operation proceeds continually, by day and by night, in sunshine and in the shade ; the other takes place only at intervals, and while the plant is exposed to the sun. Where, without light, oxygen is consumed, the plant lives and grows, but its colour and properties are impaired ; where light is present, and oxygen is produced, the colour and other properties of the vegetable attain the greatest perfection. Lastly, for the conversion of oxygen gas into carbonic acid by plants, a certain temperature is required ; but the

production of this gas, by the decomposition of carbonic acid, occurs in temperatures unequal to the support of vegetation. These numerous and striking dissimilarities must be allowed to create an essential difference between that operation of plants, by which oxygen gas is converted into carbonic acid, and that by which this acid is re-converted into oxygen gas.

333. Now the former process, by which oxygen is consumed, must be regarded as necessary to vegetation, because without air plants do not grow ; and no other known change is produced in this air than the conversion of its oxygenous portion into carbonic acid gas. This conversion, also, is connected with the living powers of the plant ; for when these powers are suspended by cold, the plant no longer continues to vegetate, and the oxygen of the air is then no longer changed. It proceeds, likewise, at all times, and in all situations ; so that if a circumstance which universally accompanies vegetation, and without which it cannot continue, be entitled to be considered a necessary condition of that function, then the conversion of oxygen gas into carbonic acid by living vegetables must be so considered.

334. But, on the other hand, the production of oxygen gas, by plants exposed to the sun, is not, necessarily, accompanied by any signs of vegetation ; neither does it require the conditions, which are essential to the exercise of that function. Thus, although air is indispensable to vegetation, yet various experiments prove that oxygen gas is afforded by the leaves of plants, when they are immersed in water. It may be said, that the air, existing in the water, is,

in this case, supplied to the plant ; but Ingenhousz asserts, that oxygengas is likewise produced by plants immersed in water, from which all the air has been previously expelled by boiling *. Nay, the experiments of Priestley, Senebier and Woodhouse demonstrate, that the production of oxygen by vegetables takes place not in proportion to the quantity of that gas which the water may contain, but to that of the carbonic acid which exists in it. It is a fact, however, that boiled and distilled waters, deprived of air, do not support the vegetation, even of aquatic plants, and water fully saturated with carbonic acid is still more destructive ; for the *conferva rivularis* and *potamogeton crispum*, says Dr Ingenhousz, were soon destroyed in it † ; and yet from such water, so impregnated in a slighter degree, he often obtained, by means of plants, a great quantity of air of exquisite purity ‡.

335. Nor is it only when they are thus secluded from air, or immersed in water deprived of air, or in that which is impregnated with carbonic acid, that this operation of yielding oxygen gas is performed by the leaves of vegetables ; for the experiments of Davy (300.) shew, that these leaves, when confined in pure carbonic acid, equally afford oxygen gas, and those of De Saussure prove the same thing to happen in vessels of hydrogen or nitrogen gas ||, although it is universally admitted that these gases are incapable

* Exper. tom. ii. p. 321.

† Exper. tom. ii. p. 270. ‡ Ibid. p. 283.

|| Recherches Chim. p. 84. et seq.

of supporting vegetation. If, therefore, we pronounce this operation of the leaves to be a vegetative function, we must maintain that vegetation takes place not merely without the presence of oxygen gas, which is necessary to vegetation, but that it actually goes on most rapidly when a gas is present, which is absolutely destructive to that process.

336. In like manner it has been shewn (24.), that heat is necessary to vegetation, and that this process does not go on if the heat be, to a certain extent, withdrawn; yet M. Senebier found, that all vegetables, which continued green, furnished oxygen gas in the sun, when the temperature of the air was many degrees below zero*. Thus, we see^d that the presence of light enables the plant to furnish oxygen gas, when the heat, necessary to vegetation, is withdrawn; and, on the contrary, it has before been shewn (25.), that all plants will vegetate in circumstances where heat is present, but where light is either partially, or totally excluded.

337. But not only without pure air and heat do plants, when exposed to the sun, seem capable of yielding oxygen; but they afford it also in such a state of mutilation, as must be completely destructive to their vegetative power. There is a great difference between the mere possession of life, and the performance of living action. Neither the stem alone, nor the leaf alone, can execute the proper vegetative function of the plant; for the stem must possess an embryo plant or bud to enable it to grow, and, though

* Physiologie Vég. tom. iii. p. 278.

the leaf may, for a long time, be kept in life, yet it neither grows, nor produces any new vegetable matter, when it is separated from the stem. In many of the experiments, however, of Ingenhousz * and Senebier †, oxygen gas was not only produced by the leaf after it was detached from the plant, but even after it had been cut into small pieces, or was so much withered as to be unable to support an erect posture. When, indeed, it was beaten to a pulp, Dr Ingenhousz did not find it capable of affording pure air; but this only shews that a certain state of vegetable organization is necessary to the discharge of this office, but does, by no means, prove, that it is performed by the vegetative function of the plant.

338. Dr Ingenhousz himself, indeed, attributed the salutary influence, which plants exert on the air, to the direct light of the sun, conjoined with a certain state or condition of the vegetable structure, but not, in any degree, to vegetation, as such. At first, indeed, he believed it to depend on vegetation; but if vegetation, he adds, were the cause, plants should produce the same effects at all times, and in all situations, where they are able to vegetate; but they live and grow, to a certain size, in darkness, where they neither afford good air, nor possess the power of improving that which is bad ‡. Since, then, it appears, that the production of oxygen gas by plants takes place independently of vegetation, and without the

* Exper. tom. ii. p. 166.

† Mem. Phys. Chim. tom. i. p. 118.

‡ Exper. tom. i. p. 58. tom. ii. p. 35.

conditions essential to the exercise of that function, it cannot be regarded as an operation necessary to the existence of the plant, or dependent on its living powers, but must be considered as a secondary and subordinate office, the performance of which is entirely governed by the accidental circumstances in which the vegetable may happen to be placed.

339. But if it be thus granted, that the operation, by which plants afford oxygen, may go on in circumstances where the powers of vegetation are not only suspended, but where they are unable to exist, then it also follows, that the entrance of gaseous fluids into plants, by which that operation is supported, is not necessarily to be considered as the result of a vital function. These gases have been shewn (318.), in certain cases, to enter plants with the fluids which they absorb from the soil; and if this, as is probable, be the ordinary way in which they gain admission, then we may say generally, that gases are *absorbed* by plants. But as these fluids may likewise be received into plants, although they hold no gases in solution, and vegetation may then also go on, the absorption of gases cannot, in this sense, be deemed a proper and necessary function of the plant.

340. Farther, gaseous bodies enter into plants, in an elastic form, and, as we have seen (300.), under circumstances fatal to the exercise of their living powers; in which case, their admission must depend on the operation of a chemical, or mechanical cause. Do we then possess evidence that such an affinity subsists between vegetables and certain gases, as is sufficient to explain the phenomena which have been

now described? That an affinity subsists between the carbon of vegetables, and the oxygen of the air, the phenomena of vegetation abundantly testify; and some experiments of M. de Saussure seem, also, to shew, that, beside converting oxygen gas into carbonic acid, certain vegetables possess an *attractive* power for that gas, under which it enters, unchanged, into the vegetable body. Thus, he remarks, that if the leaves of the *cactus opuntia* be confined, through the night, in atmospheric air inverted over mercury, they will remove a portion of its oxygen without producing an atom of carbonic acid, or, in the smallest degree, affecting its nitrogenous portion*. When the leaves have thus acquired a full dose of oxygen, they begin, says M. de Saussure, to produce carbonic acid, by yielding their carbon to unite with the surrounding oxygen,—a combination which does not, in the least, affect the volume of air. The greater number of leaves, and, in particular, those which are not fleshy, thus form carbonic acid, at the same time that they receive, or, as M. de Saussure says, inspire oxygen gas†.

341. But though the entrance of oxygen gas, to the exclusion of nitrogen, seems, in the foregoing examples, to favour the notion of the operation of an *elective* affinity; yet M. de Saussure observes, that when the *cactus* has been confined through the night in pure hydrogen gas, it has afterwards afforded, by the action of the air-pump, small quantities both of hydrogen and nitrogen gases. He also found, that

* Recherches Chim. p. 64.

† Ibid. p. 67.

where a small portion of carbonic acid was added to atmospheric air, it entered the vegetable in a proportion equal to that of oxygen ; but none entered, if the plant was confined in pure carbonic gas *. In the experiments also, which have been related (328. et seq.), not only oxygen, but hydrogen, nitrogen and carbonic gases obtained free admission into the vegetable body. The experiments likewise of Davy (300.), and of De Saussure (310.), prove the entrance of carbonic gas into the leaves of vegetables ; so that if the entrance of these gases be ascribed to chemical affinity, this affinity is not confined to one gas, but extends to many. It farther appears, that, though oxygen is generally converted into carbonic acid after its entrance into the leaves, yet it is again completely expelled in the state of oxygen gas, under exposure to the sun, and the other gases are often expelled without any sensible change in their properties, or any diminution in their quantity ; wherefore no part of them can have been appropriated by the plant †.

342. Another curious circumstance, in the history of these phenomena, is, that various gases not only enter vegetables, without regard to their nature or composition, but they do so in a quantity, which, in many cases, considerably exceeds the bulk of the containing body. The leaves of the *cactus*, when confined through the night in atmospheric air, remove a portion of its oxygen equal to three-fourths of their own volume ; and if the experiment be pro-

* Recherches, p. 70. 72. † Ibid. p. 82. et seq.

longed to thirty-six or forty hours, these leaves will have taken up a volume of this gas greater by one-fourth than their own bulk *. It farther appears, that the organic structure of the leaf is necessary to the exercise of this office ; for when the leaf is previously reduced to a pulp, it does not diminish the volume of atmospheric air, but only changes its oxygen into carbonic acid † ;—a fact which accords with an observation of Ingenhousz, who remarked, that leaves, when beaten to a pulp, did not, like those which were entire, afford oxygen gas in the sun. The leaves of various other plants, of different species, were found, by M. de Saussure, to take up a quantity of oxygen, equal or superior in bulk to themselves, when they were confined in atmospheric air and kept in darkness ‡ ; and this gas was again expelled from them when they were exposed to the sun ||. Thin-leaved plants, however, which possess a small extent of parenchymatous structure, do not at all vary the bulk of their atmosphere, when they are placed alternately in sunshine and in the shade ; and, if kept entirely in the shade, they diminish both the purity and the volume of their atmosphere §.

343. Granting, therefore, that, in the foregoing examples, chemical affinity is, to a certain extent, exerted between the vegetable and the elastic fluid that surrounds it, yet the almost indiscriminate reception of such fluids would seem to indicate that mechanical causes likewise participate in the pro-

* Saussure Rech. p. 66.

† Ibid. p. 74.

‡ Recherches, p. 81.

|| Ib. p. 82.

§ Ib. p. 91.

duction of this effect. This is rendered more probable, from the circumstance, that the organic form of the leaf is an essential condition in the operation; for, if chemical action alone were concerned, there does not appear any good reason why affinity should not in part produce a diminution in the air, even although the texture of the leaf were broken down;—a circumstance which cannot be supposed entirely to suspend or change, though it may be conceived to modify, the action of this power. It is farther remarkable, that thin leaves, although, in chemical composition, they resemble thick ones, exert little or no effect of this kind, which shews that it is not so much the quality, as the form, of the substance, that influences the operation.

344. Since, then, it appears, that, though chemical affinity be present, and allowed to operate, it is yet unequal to the explanation of the phenomena in question; let us, in the next place, direct our view to the consideration of mechanical causes. Now, mechanically considered, gaseous bodies can only be conceived to enter plants to the exclusion of some other substance. When, therefore, the vessels or cells of plants are already filled, no air can be supposed to gain admission, but by the displacement of a corresponding bulk of the contained fluids. Dr Hales, accordingly, found, that although, by means of the air-pump, he could produce a free current of air through sticks of considerable size, yet, from young and succulent shoots, placed in similar circumstances, and which yielded their fluids very

slowly, little or no air issued *. Unless, therefore, we suppose some cause capable of emptying the vessels of plants, it is difficult to conceive how the air can, simply by the force of mechanical pressure, enter and be transmitted through them.

345. All the circumstances, which have been already stated, seem to shew that the air, which enters plants in an elastic form, is chiefly, if not entirely, diffused through their cellular structure, and does not gain admission into their vascular system. Hence the hollow stems of herbaceous plants and the cellular structure of thick leaves, chiefly furnish air; and when, therefore, the texture of these parts is broken down, the vegetable substance no longer exhibits the property of diminishing or attracting air. The universal distribution of this cellular tissue through the vegetable body, its loose and porous structure, its comparative emptiness, and the ready communication that probably obtains through its whole extent, fit it, in an especial manner, for receiving and containing elastic fluids; while the closer texture of the vascular system, its less general communication, and, above all, its occupancy by the vegetable fluids, present, on every side, obstacles to the transmission of air.

346. But granting that the air, which enters in an elastic form, resides chiefly in the cellular or parenchymatous structure of plants, we have yet to investigate the cause of its entrance, and the still more unaccountable fact of its existing in the vegetable, in

* Veg. Statics, vol. i. 155. 160.

a bulk that exceeds its own absolute volume. Speaking then mechanically, no other cause of the first entrance of air into vegetables occurs, but that of pressure: and when we consider the vast force which the atmosphere exerts, in order to preserve an equilibrium, we seem to approach a cause adequate, in a certain degree, to the purpose assigned it. If, therefore, from variations in temperature, from changes in the state of the vegetable, or from chemical agency, a partial *vacuum* should, at any time, be created in the plant, air would quickly rush in to restore the equilibrium; and thus, on mechanical principles alone, the entrance of air may, to a certain extent, be explained. This explanation, however, applies only to elastic fluids generally; but does not explain why, when atmospheric air is employed, its oxygenous portion only should enter the leaf, indicating an *election*, which can be accounted for only on the principles of chemical affinity. Hence, then, we should say, that the air was brought within the sphere of attraction, by the operation of mechanical pressure, but that the actual attraction of oxygen was determined by the superior chemical affinity which the vegetable substance possessed for that gas. When, however, oxygen is not present, then nitrogen or hydrogen will enter according to their respective affinities, or, if they be considered to exert none, they will, in a certain degree, be forced in by mechanical pressure alone.

347. After the admission of such fluids, different chemical affinities may be exerted towards different gases. Thus, oxygen may be converted into car-

bonic acid, and the subsequent attraction of this acid by the vegetable substance may favour and promote its farther entrance : and such, probably, is the cause why oxygen is received in a quantity so much exceeding that of every other gas, except carbonic acid. But some other cause, beside chemical change, seems, in many cases, to operate in the production of these phenomena, else there appears no reason why the organic form of the plant should be so essential to their completion. Now, we know, that, besides being chemically attracted, air adheres to the surfaces of all bodies ; and, therefore, the more extensive the surface may be, the more extensive will be the adhesion between it and the air. To us it appears probable, that some such operation is exerted by the extended cellular structure of plants ; and therefore it is, that this structure aids the operation in question. It may, indeed, be said, that the same circumstance would enlarge the sphere of chemical action, and, by thus more extensively causing the production of carbonic acid, and its subsequent attraction by the vegetable substance, give rise to the phenomenon in question. But whether the elasticity of the air be, in these examples, overcome by this mechanical operation of adhesion, or by the effect of chemical change, or by both conjointly, we are compelled to conclude that cellular or fleshy leaves possess the property of receiving and retaining a quantity of elastic fluid, which, at the ordinary pressure and temperature of the atmosphere, considerably exceeds their own bulk.

348. This conjoined operation of mechanical and chemical causes, which we have supposed to pro-

duce the entrance of air into the vegetable body, is exhibited in the production of many other effects. Thus water is capable of receiving and containing within its pores different gaseous fluids, in very different proportions. Nitrogen gas enters in small quantity, oxygen in a larger proportion, and carbonic gas in a much larger still. Now the mechanical operation of pressure acts alike on all these gases, and, to a certain extent, causes their entrance into the water; and hence, if the pressure be artificially increased, the quantity of gas that enters is increased, or, if it be diminished, or entirely removed, the gases in part escape, or are no longer retained. But the reason why oxygen is received in larger quantity than nitrogen, and carbonic gas than oxygen, can only be explained on the supposition of a difference in the affinity subsisting between water and those gases respectively. In the same manner, the entrance of all elastic fluids into vegetables, may, to a certain extent, be deemed mechanical; but, after their admission, a chemical cause may be considered to operate. Hence it is, that, in the foregoing experiments of M. de Saussure, the oxygen gas which so largely enters the leaves of fleshy plants, cannot be abstracted by the air pump, nor, while they are kept in darkness, by placing them in temperatures equal to 40° Reaumur*; but it is entirely expelled (342.) in its primitive form and quantity, by the chemical agency of the solar rays.

349. From the facts which have now been stated

* Recherches Chim. p. 67. 69.

we collect, that plants, which vegetate in sunshine, require always the presence of oxygen gas (309.); and that, by the act of vegetation, they constantly change this oxygen (308.) into carbonic acid. We farther learn, that carbonic acid enters plants, both with the fluids which they absorb (318.), and also, under certain circumstances, in an elastic form (300.); that this acid gas is conveyed to the leaves, and is there decomposed (290. et seq.), by the joint operation of the plant and of solar light; and that it is from this source alone (317.), that the oxygen gas afforded by plants is derived. It likewise appears, that this operation of affording oxygen is not properly a vegetative function (338.), but only a subordinate office, accomplished by the direct agency of the sun; that it is carried on in the cellular or parenchymatous structure (323.), and not in the vascular system of the leaf; and that it may, and does exist (308.) with that function by which oxygen is consumed, and which is essential to the vegetation of the plant. Hence it is, that, when plants are made to grow in closed vessels exposed to the sun, the oxygen gas which is consumed by the function of vegetation, is again restored (308.) by the decomposition of the acid that is formed, and no change (306.), therefore, appears to be effected in the composition of the air. But in situations, where the direct agency of light is excluded (263.), no decomposition of carbonic acid is perceptible, and the air, therefore, soon becomes unfit to sustain vegetation. In its general nature and effects, therefore, the function of vegetation is precisely the same in sunshine and in the

shade ; for oxygen gas is alike necessary in both situations, and is in a similar manner converted into carbonic acid. Under direct exposure to the solar rays, however, this acid gas is again decomposed, and its oxygen is restored to the atmosphere ; while, in the shade, no such operation takes place, and the air, therefore, remains permanently depraved.

350. But farther, it also appears, that the production of oxygen is entirely confined to the leaves (292.) and other green parts of plants ; and that the flowers, the fruits, the stems and roots of vegetables (279.) both in sunshine and in the shade, convert always the oxygen gas of the air into carbonic acid. Since, likewise, the leaves acquire their green colour by the direct influence of the same agent as occasions the development of oxygen, may we not reasonably presume, that some necessary connection obtains between the production of that gas and the formation of this colour ? Let us then pursue these inquiries a little farther, and try to discover the nature of that relation which appears to subsist between the production of oxygen gas, and the formation of the green colour in plants.

SECT. III. *Of the Relation subsisting between the production of Oxygen Gas, and the formation of the Colours of Plants.*

351. SINCE, from the conclusions deduced in the foregoing section, it appears, that the pure air fur-

nished by plants proceeds from the decomposition of carbonic acid, and since this decomposition is effected by the joint action of the green parts of vegetables and of solar light; why, it may be asked, are these parts so exclusively concerned in this operation, and what are those peculiarities of structure or of composition, which thus enable them to produce changes in the air, so different from those which all the other parts of the vegetable perform, even in sunshine, and so contrary to their own proper functions in the shade? This operation has been shewn to be in no respect necessary to the life of plants; for they live and grow in situations where they afford no oxygen gas. Neither can it be considered as a necessary result of vegetable organisation; for the structure of white leaves, which afford no oxygen, is as perfectly developed as that of green leaves, which yield it in abundance; and when plants are successively rendered green, or white, by the alternate admission or exclusion of the solar rays, in which states they respectively furnish a pure or impure air, it cannot be supposed that the vegetable organisation has undergone such material changes as should qualify it thus rapidly to present such contrasted results.

352. This decomposition of carbonic acid, which thus gives birth to oxygen, we have likewise seen to be effected in the parenchymatous structure of the leaf, and the agency of the solar rays appears to be essential to its completion. But it is in the same part of the plant that its colourable juices reside, and these juices, also, acquire their green colour from the di-

rect influence of the sun ; so that the decomposition of carbonic acid, and the formation of the green colour, not only occur at the same time, and in the same place, but they are accomplished by the immediate operation of the same powerful agent. There exists, says M. Senebier, a very singular relation between the parts of leaves which furnish most air, and those of etiolated leaves which first become green. It is in the angles formed by the nerves of the leaves that we observe these two phenomena. It is in these that the excretory vessels of the parenchyme terminate, and it is there that light announces its operation, as we see by the air which this part affords, and by the green colour which at the same time it assumes *.

353. But not only do these operations appear thus to go on at the same time, and in the same part of the plant, but they seem to have a near connection with each other ; for it is only by the green parts of vegetables that oxygen gas is afforded ; while all their other parts yield only an impure air. M. Senebier made many experiments on the seminal leaves of French beans, and on the young leaves of other plants, but never obtained from them any oxygen gas while they retained their white colour. Neither did the etiolated leaves of lettuce or cabbage afford air when exposed to the sun, or the little which they sometimes yielded, was always very impure †. Why, then, should the decomposition of carbonic acid always attend the production of the green colour in plants, and why should their white colour appear

* Mem. Phys. Chim. t. ii. p. 98. † Ibid. t. i. p. 110. 115.

always to be accompanied by the retention of that gas? Could we discover the connection between these facts, it might, perhaps, lead us to an explanation of the cause of the green colour of plants.

354. As we have before endeavoured to shew (338.), that the decomposition of carbonic acid by plants cannot properly be deemed a vegetative function, so likewise the changes which plants exhibit, in passing from an etiolated to a coloured state, and the circumstances under which these changes take place, equally prove their independence of a living action. When an etiolated plant is gradually exposed to light, we first observe, says M. Senebier, the most tender parts pass from a white to a yellow colour; the yellow then becomes deeper; next, some green spots are seen at the extremity, and on the borders of the leaf, and in the angles of its nerves. These spots multiply, extend, and meet; the stalk of the leaf afterwards becomes green, and, lastly, the stem. The new leaves are green from the first, and thus upon the same stem we may see some leaves very green, others much less so, and a stem that is still white. In young leaves, the extremity is often greener than the other parts; and in the *narcissus* he often saw the point of the leaf more green than the rest, and the plant, which had just issued from the bulb, less green than that which had been some days exposed; whence it seems to result, says M. Senebier, that the green colour depends absolutely on a combination effected exteriorly, and which, to a certain extent, is independent of the internal vegetation of the bulb *.

* Mem. Phys. Chim. t. ii. p. 88. 90.

355. In prosecution of this idea, he procured a piece of tinfoil, an inch square, and with it covered a portion of the green leaf of a narcissus. In a few days, the leaf still appeared green over its whole extent, except in the part covered by the tinfoil, which part was very yellow. So, likewise, if an etiolated leaf of the same plant was covered with a piece of tinfoil, and then exposed to the sun, the whole leaf gradually became green, except in that part covered by the metal *;—facts which not only demonstrate the direct influence of the solar rays, but likewise the locality of their action.

356. In like manner too, as we have seen green leaves to produce oxygen gas in sunshine, after their separation from the stem, so do etiolated leaves become green, when placed in the same circumstances. M. Senebier exposed, in his window, the etiolated leaves of the hyacinth, to the direct rays of the sun, and in a few hours they became green; but the thin leaves of other plants did not exhibit this change of colour, evidently because they became dry before it could take place. When, also, etiolated plants are exposed in water to the sun, they become green, in the same manner as leaves afford oxygen gas, when placed in similar circumstances. He has seen the leaves of French beans, which sprang white out of the earth, become sensibly green in an hour, under exposure to an ardent sun †. Not only, therefore, do the mode and circumstances in which the green

* Mem. Phys. Chim. vol. ii. p. 90.

† Ibid. p. 78. 91. 93.

colour forms in leaves, but the rapidity with which it is effected by the direct action of the sun, shew it to be accomplished by a power which acts locally and exteriorly to the plant; and whose operations are carried on in a manner distinct from those which properly constitute vegetation. These facts, therefore, supply another coincidence between the decomposition of carbonic acid, and the formation of the green colour in plants, and still farther confirm the immediate relation that appears to subsist betwixt them.

357. To account for the green colours of vegetables, various hypotheses have been proposed. M. Humboldt, having observed some growing plants to retain their green colour in mines, from which light was excluded, and in which the atmosphere was impregnated with hydrogen gas, was led to ascribe the production of the green colour to the operation of hydrogen *. It is evident, however, that plants become green in the common atmosphere, where little or no hydrogen gas is present; and M. de Saussure, in various trials, could never observe that plants became more green from the languid vegetation which they experienced in vessels of hydrogen gas †.

358. M. Humboldt farther attributed the white condition of the leaf to the operation of oxygen; and we have seen, that the leaf becomes green only when oxygen gas is expelled from it. But the non-expulsion of oxygen is no proof that the white colour is caused by the operation of that substance;

* Thomson's Chemistry, vol. v. p. 362. 4th edit.

† Recherches, p. 210.

for, previous to its expulsion, this oxygen exists in union with carbon, and, therefore, as oxygen, it cannot be considered as producing the white colour in question.

359. From the circumstance of carbonic acid being decomposed, and its oxygen only expelled, when plants become green, M. Senebier, in conformity with the opinions of the day, was, at first, led to ascribe this colour to the operation of phlogiston*, and subsequently, to the retention of carbon, which he supposes to be largely deposited in the parenchyme of the leaf†. But, granting that carbon is thus retained in the leaf, we possess no evidence that it is able to produce the green colour. The juice which communicates this colour is contained in the cells of the parenchyme. It is of a resinous nature, and soluble in alcohol, to which it imparts its green hue: and this green solution possesses the same properties, and exhibits the same changes, from whatever leaves it is obtained. If a phial, about one-third filled with this solution, be exposed to the direct light of the sun, its green colour is discharged in about twenty minutes, and the liquor resumes nearly the transparency of alcohol, except that it is a little tinged by the vegetable matter, while a yellowish precipitate is at the same time thrown down‡. In what manner, then, is this discharge of the green colour produced?

* Mem. Phys. Chim. passim.

† Phys. Veg. t. iii. p. 158.

‡ Mem. Phys. Chim. t. iii. p. 6.

360. The heat that exists in the solar beam, is not, says M. Senebier, the cause of this change of colour; for, when light is excluded, the green tincture preserves its colour for many months, although it be exposed to a considerable degree of heat. Even at the temperature of 60° Reaum. the colour suffers no change, if the solution be kept in darkness; so that light, not heat, is the agent concerned in producing this change. But though light thus accelerates this change, yet its direct influence is not essential to its production; for the green colour will be gradually, but much more slowly, discharged in the shade*.

361. Besides light, however, air also is necessary to the success of the experiment; for vessels, which are quite filled with green solution, and closely stopped, suffer no change of colour for many months, even in sunshine: and those portions of solution, which present the greatest surface to the air, or which are contained in phials with the largest quantity of air, experience the greatest change; so that the discolouration is always proportional to the quantity of air that is present†. It is, however, only the oxygenous part of the air that contributes to these changes; for when confined in phials which contain only nitrogen gas, the solution experiences no change, although it be exposed to the sun; but when oxygen gas is present, the discolouration is then more rapidly effected, than by common air alone‡. It follows from these facts, that the presence of oxygen gas, and the agen-

* Mem. Phys. Chim. t. iii. p. 8. 9. 13.

† Ibid. p. 15. 16. ‡ Ibid. p. 18.

cy of light, are conjointly required to produce these changes of colour in the green solution : and, in conformity with his hypothesis, concerning the cause of the green colour of plants, M. Senebier supposes the oxygen to be necessary to carry off the carbon, on the presence of which substance, he believes this colour to depend *.

362. The observations of Senebier were repeated by M. Berthollet, who remarks that the green colour of the solution disappears rapidly in sunshine, more slowly in the shade, and not at all, or very slowly indeed, in perfect darkness. To discover the effects produced in the air, he inverted a vessel, half filled with green solution, over mercury, and exposed it to the light of the sun. When the colour was discharged, the mercury was found to have risen into the vessel : and, consequently, says M. Berthollet, the oxygen had combined with the colourable parts of the solution †. He did not observe the precipitation which M. Senebier mentions, but the solution continued transparent, and of a clear yellow colour. When

* *Physiol. Veg.* tom. iii. p. 175.

† Authors, in general, have denominated the matter which, in these experiments, affords colour, the colouring parts of the vegetable. This matter, however, is not itself coloured, but is only capable of exhibiting colours, by the addition of other matters : and hence we have ventured to call it the *colourable*, rather than the *colouring* parts of the plant, by which we merely indicate its property of becoming coloured, but not its actual possession of colour. When, by the addition of other matter, it is made to assume colour, and is, in that state, employed as a dye or a pigment, it may then be called colouring matter.

evaporated, however, the colour deepened, and passed to brown, affording at length a black carbonaceous residuum. If the vessel contained no oxygen, light did not act on the colour of the solution, and nitrogen gas experienced no diminution. Light, therefore, says M. Berthollet, favours the attraction of oxygen, and the *combustion* of the colourable matter, which at first is imperceptible, and gives to the solution only a yellow colour; but soon afterwards it is perfected by the action of heat, and the liquor becomes brown, and leaves a black residuum *.

363. With all due deference, however, to the opinions of this eminent and philosophical chemist, we must venture to protest against the employment of the term *combustion*, to express that combination which oxygen, in these instances, is supposed to form with the colourable matters of the solution; for neither in the circumstances of their union, in the phenomena which attend it, nor in the results which are afforded, does this combination at all resemble the ordinary process of combustion. If that process be rightly defined, the combination of oxygen with certain bodies, accompanied by the phenomena of light and heat, and terminating in the destruction of the properties of the combustible matter, then no process in which such phenomena are not observed, and such results afforded, can, with propriety, be named combustion. If the mere union of oxygen with combustible matter were sufficient to constitute combustion, then the living functions of vegetation

* Elemens de l'art de la Teinture, vol. i. p. 54. 55.

and respiration, and the several processes of fermentation and putrefaction, would all fall under the same denomination ; but who, that duly considers the nature, the purpose, and end of these several operations, would desire thus to confound them, under the general title of combustion? It is evident that the black residuum which M. Berthollet seems to consider as evidence of actual combustion, was not the result of the natural combination of these bodies, but of the artificial heat to which he submitted them in the process of evaporation.

364. Dr Bancroft, in his valuable treatise on the “Philosophy of Permanent Colours,” remarks, that it cannot be M. Berthollet’s intention to apply the term *combustion*, to alterations which result from a simple addition of oxygen to colourable matters, without a destruction or separation of any of their component parts. Various acids, says he, which contain oxygen, weaken or extinguish colours, not, however, by any effect which can properly be termed combustion ; for none of the colourable parts are destroyed or carried away, and the addition of an alkali restores the original colour. Many dyed substances, he adds, have their colours dissipated by exposure to the sun and air, without any other change of tint than the simple diminution of their original body, or quantity of colouring matter ; so that the cloth is left as white as before it was dyed, without any thing like combustion having ever taken place in it, or in the matter with which it was dyed *. Instead,

* Philos. of Perm. Colours, vol. i. p. 49, 50.

therefore, of considering the green colour, in Senebier's experiments, to have been destroyed by combustion, he supposes it to have been effected by the operation of oxygen, and conceives the precipitate that was formed to have been the colourable matter saturated with oxygen *.

365. This opinion, that the discharge of colour depends on the simple addition or combination of oxygen, appears to us not less gratuitous than that of M. Senebier, which considers it to arise from the escape of carbon; for, though it be granted that oxygen disappears, yet we are not told into what combination it enters, nor in what way it acts on this colourable matter. Dr Bancroft himself, in his observations on Berthollet's doctrine, remarks, that the muriatic acid discharges colours in the same manner as other acids, although it has never been proved to contain oxygen; or, if it do, this oxygen is united by an affinity too powerful to be overcome by any known substance or means. When, therefore, says he, this acid changes or destroys colours, it must be by producing effects different from those of combustion; and as these changes are in most cases similar to those produced by the other acids, which contain oxygen, it seems reasonable to conclude, that these also act upon colours by producing other effects than those of combustion †. If, however, this reasoning be valid against the hypothesis of combustion, because the muriatic acid cannot afford oxygen to carry on that process, it must, for similar reasons, apply equally against Dr Bancroft's hypothesis, which ascribes the

* Philos. of Perm. Colours, vol. i. p. 43. † Ibid. p. 51.

discharge of colour to the simple combination of oxygen.

366. But, if it thus appear, that the muriatic acid discharges vegetable colours, like other acids, although its oxygen cannot be separated, may we not presume that these other acids act not in virtue of the oxygen which they contain, but by reason of their acid properties alone? And since we also know that oxygen gas readily combines with the carbon of vegetables, and forms an acid substance, may we not farther presume, that the oxygen, employed in these experiments, really entered into such a combination, and that the acid, thereby formed, contributed to the discharge of the vegetable colour?

367. That carbonic acid is really formed, when oxygen gas is placed in contact with this green solution, seems almost certain from the following fact. If, says M. Senebier, sulphuric acid be poured into a green solution, which has been previously discoloured by light, it occasions a strong effervescence, the mixture then reddens, and a white precipitate is thrown down, similar to that which the green tincture affords, when, in its coloured state, it is made to combine with the same acid*. There can, we think, remain little doubt, but that the elastic matter, which is thus driven off by sulphuric acid, is carbonic acid gas; and hence, when oxygen gas disappears in these experiments, and combines with the colourable matter, the discharge of colour that ensues is not properly to be ascribed, as M. Senebier supposes,

* Mem. Phys. Chim. vol. iii. p. 44.

to the simple removal of carbon, nor, as Dr Bancroft believes, to the simple addition of oxygen, but rather to the action of the acid compound, which these elementary bodies compose.

368. M. Bergman is said to have proved that alcohol attracts double its bulk of carbonic acid; and, therefore, this acid will necessarily be attracted by these solutions, as fast as it may be formed. To prove that, like other acids, it is able to discharge their colour, we inverted a bottle, filled with green solution, in the pneumatic trough, and passed up into it a stream of carbonic acid, as it issued from the mouth of a retort, that contained carbonate of lime and diluted sulphuric acid. A considerable portion of the solution was allowed to escape, and the colour of the remainder was rapidly discharged, so that it was reduced nearly to a colourless state: and when suffered to remain at rest, a flocculent precipitate subsided, and left the liquor perfectly transparent. A similar effect was produced, but much more slowly, by breathing through a tube into a bottle which contained a quantity of green solution; and as nitrogen gas has been shewn (361.) to effect no change in the solution, the discharge of its colour must also, in this instance, have been caused by the action of carbonic acid gas. Thus, then, we see, not only that carbonic acid is formed by the solution, when it is exposed to the light and air, but that it possesses, also, the power of discharging its colour, in circumstances where it must be considered to act by the exertion of its acid properties alone.

369. This conclusion is farther supported by the

fact, that the colour which is discharged, when the green solution is exposed to light and air, is again restored, according to M. Senebier, by the addition of an alkali * ; and we found, by experiment, the same restoration of colour to be made when an alkali was added to the solution which had been deprived of colour by the direct application of carbonic acid. The green colour, however, did not resume its former intensity ; and M. Berthollet remarks, that when the green solution has undergone all the discolouration from exposure which it is able to exhibit, alkali then produces in it no change †.

370. The phenomena afforded by these solutions of the colourable matter of vegetables in alcohol, agree with those which are presented by infusions of the same substances in water. The flowers of most plants, and also the leaves of many, yield to water materials which become red or green, according as acid or alkaline matter is made to predominate in them ; but if the acid and alkali be furnished in certain proportions, the colour disappears altogether.

371. Not only, however, is the green colour of vegetable infusions destroyed by the direct application of acids, but, like that of the green solutions, it is gradually discharged by exposure to the air. M. Becker remarked, that if a vegetable infusion, that has been made green by alkali, be exposed to the air, it gradually passes to a yellow colour ‡. This,

* Mem. Phys. Chim. tom. ii. p. 152. and tom. iii. p. 47.

† Elem. de la Teinture, vol. i. p. 54.

‡ Berthollet's Elem. de la Teinture, vol. i. p. 64.

however, is not generally true; for we found the blue infusion of red cabbage, which had been first rendered green by an alkali, to acquire a red colour by exposure to the air. Indeed, it is reasonable to expect, that different plants should furnish colourable juices which are variously affected by chemical agents, since not only do vegetables vary in their properties from each other, but their colourable matter is often extracted from different parts of the plant. These colourable matters not only possess peculiar properties, but differ essentially from each other, says Dr Bancroft, and must therefore be applied in different ways, and with very different means, to produce permanent colours in other matters. Many species, however, of these colourable matters suffer, he adds, nearly similar changes from the action of acids, alkalis, and other chemical agents; from which it may be presumed, that there is something of a common or similar nature in many of them *.

372. That the discharge of colour in the foregoing examples, proceeded from the formation of an acid, seems to follow from the experiments of M. Berthollet. He placed portions of tincture of turnsole in contact with oxygen gas, over mercury, both in darkness and in the light of the sun; the first portion continued a long time without alteration, and without producing any diminution in the air, but the second lost its colour and became red; the oxygen gas had in great part disappeared and a little carbonic acid was formed, which, doubtless, says M. Ber-

* On Perm. Colours, p. 72. 3.

thollet produced the change of colour from blue to red *. Hence, therefore, we learn, that vegetable infusions, like the solutions before mentioned (367.), form carbonic acid, when they are placed in contact with oxygen gas; and that this acid, when thus formed, is able to discharge their colour.

373. Thus, then, it appears, that the phenomena exhibited by solutions of the colourable parts of vegetables in alcohol and in water very nearly agree; that both are rendered respectively red or green by the predominance of acid or alkaline matter; and that, according to the proportions in which these ingredients exist, various intermediate tints are produced. Even the pale liquor, obtained by digesting etiolated leaves in alcohol, was found by M. Senebier to become green by the addition of alkali †; so that these leaves contain resinous or colourable matter similar to that which green leaves afford, and the chief difference between the two solutions is in their proportion of alkaline matter.

374. But acids and alkalis not only change the colourable matter of vegetables, when it is extracted by alcohol or water, but they act also on the entire leaf. Sulphurous acid quickly discharges the colour of green leaves, and when these leaves are plunged in the vapour of nitric, muriatic, or sulphuric acids, they pass rapidly from a green to a yellow colour. Etiolated leaves suffer at first no change, but at length become whiter. Similar effects are produced in green

* Berthollet's Elem. de la Teinture, vol. i. p. 56.

† Mem. Phys. tom. ii. p. 152.

leaves, when immersed in water charged with these several acids ; for they exhibit, at first, a fawn colour, and then become white ; but etiolated leaves are not sensibly affected *. We observed, in some instances, that the green leaf not only became yellow or brown in acid liquors, but the liquor itself acquired a reddish hue. These effects of acids on the colours of leaves are also visible in common culinary processes ; for waters, denominated hard, from containing an excess of acid, greatly discolour the vegetables which are boiled in them ; and the green colours of pickled vegetables are very much degraded by the action of the acid liquor in which they are preserved.

375. As acids are thus found to discharge the green colour of plants, so alkalis may be expected to improve it ; and experience confirms the expectation. If green leaves, says M. Senebier, be plunged in alkaline liquors, they preserve their colour, and the liquor acquires a greenish tint. Leaves rendered yellow by decomposition become brown in alkaline fluids ; but etiolated leaves, and those which are reddened by age, pass to a green, though this did not happen to the etiolated leaves of the French bean †. We observed, that if water of potassa, or muriate of soda, was added to the water in which green leaves were infused, it improved their colour ; but if the green colour was first degraded by the action of the water, these substances did not restore it.

376. If, then, it be established, by the foregoing

* Mem. Chim. tom. ii. p. 147.

† Ibid. p. 149.

facts, that the matter which gives rise to the green colour of plants (359.) resides principally in the juices of their leaves; if these juices, when extracted by water or alcohol, exhibit, in various instances, yellow, brown, red and green colours (368. et seq.), according to the proportion of acid or alkaline matter which they may contain; and if, farther, the entire leaves themselves (374. 5.) exhibit similar changes of colour when exposed to the operation of the same agents; may we not reasonably presume, that these same agents, if present, will exert a similar action on the leaves during their growth? Let us then inquire whether the actual condition of green and of white leaves authorises us to conclude that they possess corresponding proportions of alkaline and acid matter.

377. With respect to green leaves, it is known, that these, and other green parts, afford alkali in much larger quantity than any other parts of the plant. From a report made to their government by the French chemists, it appears, that herbaceous plants afford five times more ashes than large trees, and that the leaves of trees yield more than their trunks*. Alkaline salts, says M. de Saussure, form, beyond comparison, the most abundant ingredient in the ashes of herbaceous plants. In many instances, the ashes of young leaves, growing on a bad soil, contained at least three-fourths of their weight of alkaline salts; the young leaves of trees also contained half, or sometimes three-fourths, of their weight of these salts, but

* Murray's System of Chemistry, vol. ii. p. 80. 1st edit.

the proportion diminished as the plant advanced in age*. These facts prove that the green parts of vegetables contain saline compounds in great abundance: and since it has been shewn, that the colourable juices of leaves (369.), and the leaves themselves (375.), have their green colour improved, or are even changed from white to green, by the addition of alkaline matter, the same chemical changes must occur in the living leaf, if means can be found to decompose its saline compounds, and thus, by releasing their acid part, to occasion an excess or predominance of alkaline matter.

378. Now the decomposition of carbonic acid in plants, by the agency of solar light, seems to be the mean employed by nature to accomplish this purpose; for, by this mean, the acid is not only withdrawn from its combination and expelled, but the alkali is, at the same instant, rendered predominant, and exists in a state fitted to exert its specific action on the colourable juices of the leaf; and this action, as we believe, it does exert, and the leaf, in consequence, exhibits a green colour. The colouration of the leaf, therefore, is not immediately owing to the expulsion of oxygen, nor even to the subtraction of carbonic acid, but to the predominance of alkaline matter which this subtraction of acid occasions; consequently, the verdure succeeds to the decomposition of carbonic acid, and the evidence of that decomposition is the expulsion of oxygen gas. Hence, therefore, to speak correctly, we cannot so properly

* Recherches, p. 285.

say, that the green leaf affords oxygen, as that it becomes green when that gas is expelled; and thus it is that the decomposition of carbonic acid by solar light gives rise at once to the production of oxygen gas, and to the formation of the green colour in plants.

379. The relation which we have thus traced between these operations, enables us to explain why the expulsion of oxygen appears to take place only from the green parts of plants, since it is only when oxygen is expelled that those parts acquire a green colour; why light is equally necessary to the expulsion of oxygen and to the production of the green colour, because alkaline matter cannot be rendered predominant, and produce this colour, unless carbonic acid, which affords the oxygen, be first decomposed; and, lastly, why no oxygen is produced, and no green colour is formed in darkness, because no carbonic acid is then decomposed, and its presence suspends the action of the alkaline matter. The mode, however, in which these operations are carried on in the leaves, the circumstances in which they take place, and the agents by which they are effected, all conspire to prove that the processes are purely chemical, and proceed in a manner entirely independent of those functions, which contribute to the evolution, the growth, and nutrition of the plant.

380. Such is the view, which, in the progress of our researches, we have been led to form of the causes which influence and produce the green colour of plants. To us they appear sufficient to account for the phenomena; and they are, indeed, so familiarly and precisely illustrated by the well-known

changes of colour which vegetable infusions exhibit from the action of alkaline matter, that we much wonder the opinion has so long slumbered in obscurity, although, from the erroneous views which have prevailed concerning the changes which vegetables produce in the air, its full development could hardly be expected. An opinion, indeed, approaching to that which has been now delivered, seems to have occurred, more than a century ago, to M. Geoffroy, who attributed the green colour of vegetables to the combination of a highly rarefied oil with the fixed and volatile salts of the sap; and this opinion he was led to form, from finding that a solution of the essential oil of thyme in alcohol became green by the addition of oil of tartar*.

881. But M. Geoffroy, says M. Senebier, in his remarks on these experiments, did not see that the alkali alone always produces these effects upon the etiolated parts of plants, upon their green solutions which have been discoloured by light, upon their essential oils, and even upon the pale and yellow tinctures of etiolated leaves†. It is true, that, by considering this rarefied oil as necessary to the production of colour, M. Geoffroy departed from, or rather fell short of the truth, which is the more to be wondered at, as he was well aware of the effects which the same alkalis produce in ordinary vegetable infusions. If, however, it be allowable to substitute the term colourable matter for essential oil, or to consi-

* Mem. de l'Acad. Roy. an. 1707.

† Mem. Phys. Chim. t. ii. p. 176.

der the elementary nature of these substances as nearly the same, then the opinion of M. Geoffroy will come near to a true expression of the fact. It is, we think, much more remarkable, that M. Senebier, who saw all that he has mentioned, and much more that we shall have to relate, was yet turned aside from the conclusion to which his experiments naturally led, by the influence of an hypothesis which had nothing better than a name for its support.

382. BUT if the colour of green leaves depend, as we have supposed, on the predominance of alkaline matter (378.), that of white leaves may reasonably be presumed to arise from the deficiency of it; and the experiments of M. Senebier prove that such is the fact. Not only did he find alkaline matter to be less abundant in etiolated than in green leaves, but to exist in a more neutralized state; and may not, says he, this neutralization be produced by the union of the carbonic acid, in such leaves, with the alkali which they contain*? This acid he actually found, by other experiments, to abound most in etiolated leaves†; and his results are confirmed by those obtained by Davy (25.) and others. Since, therefore, etiolated leaves not only contain less alkali than those which are green, but this alkali is neutralized, or even supersaturated, by a predominant acid, it is not to be expected that it should produce its usual

* Mem. Phys. Chim. t. ii. p. 166.

† Ibid. 169.

effects ; and such leaves, therefore, like green infusions which have been exposed to the air or mixed with acids, will, from a similar cause, experience a similar degradation of colour, and exhibit only a white or yellow hue.

383. But, farther, if carbonic acid thus superabound in etiolated leaves, and destroy their colour, by modifying or overpowering the action of alkaline matter, this etiolation must continue as long as the acid is thus retained ; and this must happen as long as the plant is kept in darkness, since little air is afforded by etiolated leaves, and that little is always impure *. Hence, therefore, the etiolated state of plants depends immediately on a deficiency of alkaline, or on a superabundance of acid matter, by which the usual operation of the alkali is reduced or counteracted : but the sun's rays, by withdrawing and decomposing this excess of acid, enables the alkali to resume its former action, and thereby to restore the green colour of the leaf. This view, therefore, corresponds with the local operation of light (355.), in the restoration of the green colour ; with the fact of the formation of this colour in leaves (356.), in which vegetation is necessarily suspended ; with the results of those experiments (375.), in which white leaves were rendered green, by immersion in alkaline fluids ; and also with those, in which the white juices of such leaves (373.) were made green, by the addition of alkaline matter.

384. These views, respecting the colouration of

* Mem. Phys. Chim. tom. ii. p. 155.

leaves, seem also to explain, not only why they lose their verdure in darkness, but the cause of that degradation and change of colour which they experience in autumn, at the period, and during the time of their fall. There are many reasons which make it probable, that alkaline matter is constantly conveyed into the plant, during every period of its growth, and is thus constantly supplied to act on the colourable matter of the leaves. When, therefore, vegetation fails, this alkali will be less abundantly supplied, and the acid will proportionally prevail. But not only will the alkaline matter, at this period, be thus diminished, but the quantity of acid will be increased; for, as vegetation declines or ceases, spontaneous decomposition will begin, and the acid matter thereby developed will counteract or overpower the alkali; so that, according to the different proportions in which these ingredients meet, the colourable juices of the plant will be differently affected, and will thus exhibit those various brown and yellow tints, which compose an autumnal scene.

385. But the green colours of vegetables, not only pass to a yellow or brownish hue, but sometimes change to red. The leaves of the oak, of the pear, the vine, and barberry, often exhibit this colour: and, among herbaceous plants, the leaves of buck-wheat, strawberry, and amaranth do the same. It is only when they have attained to maturity, or are about to fall, or when they have been injured by insects, says M. Senebier, that they present a red colour. In those leaves which turn red before they fall, the redness first appears in the stalk; in others, the redness

begins at the summit, and, spreading over the surface, gradually reaches the stalk. The leaves of some plants pass from green to yellow, and afterwards become red; those of other plants become red only when exposed to the sun, and assume a yellow colour in the shade; while others pass from red to yellow, after they have fallen. It often happens, that many leaves which usually redden, become dry without exhibiting this colour; and apparently without regard to the immediate influence either of light or of heat. In all cases, red leaves lose their redness after a certain period, passing to yellow and white, and the direct action of the sun seems to accelerate these changes*.

386. In this transition from a green to a red colour, the nerves and stalks of the leaves never redden. As these parts were not previously green, it results, says M. Senebier, that it is the green matter alone which suffers this change of colour; and hence the stalks of the *melampyrum*, which were previously green, become red like the other parts of the leaf. In general, the superior surface of the leaf first changes colour, and often passes through various tints into yellow, before the inferior surface ceases to be green†.

387. The leaves, which have thus become red by age, yield to water a yellowish red or brick colour, and a red one to alcohol. The green leaves of the Canadian vine impart their green colour to alcohol, but the red leaves of the same plant tinge it red,

* Mem. Phys. Chim. t. iii. p. 69, 72. † Ibid. p. 73.

which passes afterwards to orange. If sulphuric acid be poured into the green solution, it reddens it; and the red tincture is rendered green by alkalis. The red leaves of this vine lose their colour in water charged with sulphuric acid, but the water acquires a lively red tinge; and the same leaves, immersed in an alkaline solution, have resumed almost their primitive green colour*. As thus the leaves of the same plant, in their green and red states, yield their respective colours to alcohol; as these differently coloured solutions pass to red or green, according as acid or alkaline matter is made to predominate in them; and as the entire leaves themselves exhibit similar variations in colour, when exposed to the operation of the same agents, we may fairly conclude, that the several tints which these leaves exhibit, in the successive periods of their growth, maturity, and decay, depend essentially on analogous changes, which take place in the condition of their colourable juices, according as these juices are modified by the operation of those causes, which vary the state and proportions of their acid and alkaline matter.

388. This redness of autumnal leaves, M. Geoffroy, indeed, ascribed to an acid developed at that period, which, by overpowering the alkali, gave rise to the red colour, in the same manner as distilled vinegar changes a green solution to a red colour†. And M. Senebier himself, who, as we have seen, attributes the

* Mem. Phys. Chim. t. iii. p. 77. et seq.

† Mem. de l'Acad. an. 1707.

green colour to a cause different from that of an alkali, nevertheless ascribes the red colour, in these instances, to the predominance of acid matter, which is developed under that incipient decomposition which the leaves, at the decline of vegetation, experience *. The production of acid matter by vegetable infusions, in their transition from a green to a red colour (372.), leads M. Berthollet also to believe, that an acid may be developed in those leaves which, in autumn, redden before they pass to yellow †.

389. But besides this red colour, which the leaves of different plants exhibit, in the more advanced periods of their vegetation, there are other leaves, says M. Senebier, which proceed red from their buds, and become green only in the latter periods of their growth; such are those of the apricot, the walnut, the maple, and the pear, the leaves of which last plant are of the same colour in their earliest and most advanced age ‡. In these instances, in which the young leaves are at first red, it is easy to suppose, that the colourable juices contain an excess of acid, which enables them to exhibit this colour; but by the action of the solar rays, this excess, at a later period, is reduced, and the alkali becoming finally predominant, gives rise to its accustomed hue. In the young leaves of most buds, the colour is only white, indicating a less proportion of acid; and these leaves,

* Mem. Phys. Chim. vol. iii. p. 83. 85.

† Elemens de la Teinture, vol. i. p. 57.

‡ Mem. Phys. Chim. t. iii. p. 75.

too, become green under a similar exposure to the solar rays.

390. But although the light of the sun thus contributes to produce the green colour of plants, yet, as it operates chiefly by decomposing acid matter, and thereby reducing its predominance in the colourable juices of the leaf, it follows, that its agency is not essential to this production of colour, if the alkali, from any other cause, can be rendered predominant in those juices. Thus, when the colourless solutions of etiolated leaves, or the colourless infusions of flowers, are rendered green by alkalis, the agency of light is no way concerned in producing the effect; and so, likewise, if the alkali, in living plants, can, in any way, be rendered predominant, under circumstances where light is excluded, still the colourable juices of the leaf may, nevertheless, be expected to exhibit more or less of a green colour. M. Senebier remarks, that the leaves of etiolated French beans often exhibit a green appearance at the part where they are connected with the stalk; and this appearance is seen at the commencement of vegetation, while the seed is yet buried in the earth, and before light, therefore, can have come into contact with it *. So, likewise, the buds of the chesnut, while enveloped in their thick and gummy cases, possess a green hue before their development; whence, says he, it is not to be doubted, that vegetables, and parts of vegetables, may be coloured green, although light does not seem to act immediately upon them †.

* Mem. Phys. tom. ii. p. 86.

† Ibid. p. 97.

391. We have already seen, that, in a very weak and diffused light, some plants appear to decompose carbonic acid, and to retain their verdure; and many leaves also, which grow in the shade, are not less green than those which are fully exposed to the sun. All such facts may be easily explained, on the supposition that alkaline matter naturally superabounds in the plant, and produces its specific effects on their colourable juices; while, in other cases, the direct agency of light is more or less necessary to withdraw and decompose the acid, whose presence prevents the appearance of the green colour. It is, however, highly probable, as M. de Saussure observes, that carbonic acid is decomposed in plants, even in a very weak light (312.), although, from the slowness of the operation, and the immediate consumption of the oxygen by vegetation, the fact cannot be easily detected. Such an opinion permits us to suppose a redundancy of alkaline matter, without resorting to the less obvious supposition of an unusual supply of it, to counteract the effects of the retained acid.

392. Besides those transitions from red to green, which the young leaves of certain plants have been shewn to exhibit, there are other leaves which present contrary characters, being at first nearly green, and becoming at length perfectly and permanently red. The red cabbage affords an example of this kind; and a comparison of its peculiarities with those of the green variety may assist us in explaining its cause. In the common cabbage, all the leaves which are exposed to light, acquire a green colour, while those in the centre or heart of the plant have but lit-

tle of a greenish hue, or are entirely white. These white leaves, however, have been shewn (373.) to contain the same colourable juices as those which are green, and, when extracted by alkohol, they pass to a green colour by the addition of alkaline matter. Hence the colourable matter is more or less distributed through the whole plant; and, in the manner already stated, it is rendered white or green according as the plant, or any part of it, is exposed to, or secluded from light. This colourable matter, as we before remarked, is chiefly contained in the parenchyme of the leaf, and the colour is transmitted through the epidermis which invests it.

393. In the red variety, however, the colour is not communicated by the juices of the parenchyme, but by the outer skin or epidermis itself; while the parenchymatous juice beneath is yellow or white. The redness, too, of this plant is seen both in those leaves which are secluded from light, and in those which are exposed to its action; consequently this agent exerts no immediate effect in their colouration. The leaves of this cabbage yield a bluish tint to water, which, like that of turnsole, is rendered red by exposure to the air, or by impregnating it with carbonic acid. In the same manner, it is rendered green by alkalis, so that the red cabbage must contain a colourable matter, similar to that which is diffused through the green and white leaves of the common variety. This, indeed, is easily seen by inspecting the plant itself; for though the skin of its leaves is red, yet the parenchyme of the outer leaves is, in many parts, sensibly green, and transmits even a

dusky green hue through the red covering which invests it. So, likewise, those portions of leaf, which have been infused in water or alkohol, and thereby nearly deprived of colour, become red or green, according as they are immersed in acid or alkaline liquors; and the same may be said of the substance of the parenchyme, from which the epidermis has been previously removed. Hence the same colourable matter resides in the leaves of the red cabbage as in those of the green variety, as is thus manifested by its undergoing the same effects, when exposed to the operation of the same agents. Consequently, the difference of colour in the two varieties is to be sought in that of the epidermis alone.

394. Now, the cause of the red colour in this epidermis appears to be that same predominance of acid, which occasions it in other instances; for the red tint, which these leaves afford to alkohol, is not affected by exposure to the air, but is exalted by acids, and rendered green by alkalis; and these transmutations of colour may be indefinitely repeated. If, farther, the epidermis be raised, and carefully removed by dissection, it will be changed instantly to a rich green colour by immersion in an alkaline liquor, and may be again restored to a red hue by plunging it in an acid; but if the immersion in alkali be prolonged, the colourable matter dissolves, and the green tint passes into yellow. If, also, pieces of the entire red leaf be immersed in acid liquors, the edges of the parenchyme soon change from green to red, and the colour of the epidermis is at the same time considerably heightened; while, in alkaline fluids, simi-

lar portions of leaf have the green hue of their parenchyme improved, and even the red epidermis finally assumes a green colour. This alternate change and exaltation of colour, according as acid or alkaline matter is made to predominate, authorise us to ascribe the production of colour, in the epidermis of these leaves, to the same excess of acid which imparts the red tinge to autumnal leaves (387.), and to the young leaves of certain other plants (389.), although the seat of colour be placed in a different part of the vegetable structure.

395. In the same manner, the colour of certain roots, as of the radish, of some varieties of potatoe, and of the turnip, seems to reside in the outer coat, while, in the carrot and beet, it is diffused through the substance of the root. In the radish, the colour extends over almost the whole root; in the turnip, it is confined to that part which is exposed to light; and even the radish, where it comes into contact with light (286.), assumes a purple hue. Now, if light operate in the way that has been stated, it will variously affect the colours of these parts, according to the degree of its action. It is, as we have seen, in the green parts alone that oxygen is largely expelled; in other words, that carbonic acid is largely decomposed; and, therefore, it is in those parts only that the alkali is enabled to exert its full effect. But if the light act in a less degree, and decompose less acid, the alkali will be less predominant, and impart not a full green, but some modification of purple.

396. In illustration of these views, we may re-

mark, that the skin of the turnip, beneath the soil, where light cannot act, is entirely white, because the alkaline matter is fully saturated with acid ; in the part above ground, the colour is purple from a partial decomposition of acid ; and in some parts, it becomes nearly green as more acid is decomposed, and the alkali exerts a fuller degree of action. We have, also, often observed different parts of the same leaf of this plant to exhibit green, yellow, red, and purple colours, from accidental circumstances, which determined, as we suppose, in different degrees, the predominance of acid or alkaline matter. Thus, too, in the root of the radish, where it is entirely secluded from light, the acid may be conceived to prevail ; but exposure to light effects a partial decomposition, which reduces the excess of acid matter, and permits the purple tint to appear. This series of changes is well shewn by an infusion of red cabbage, which has, at first, a bluish tint, but passes through various grades of purple to a confirmed green, by successive additions of alkaline matter : or, if an acid be employed, the same infusion is gradually heightened to redness, and may be again brought back to purple by adding to it alkaline matter. And since the action of light, in the colouration of plants, has been shewn to be entirely local (355.), it follows, not only that a succession of colours may thus be produced in the roots or leaves, but that various colours may, at the same time, be present in different parts of the same leaf or root, according as it is more or less subjected to the influence of light, and, probably, according as the structure of the plant, and the qualities of its co-

lourable matter, more or less favour or modify the operation of this agent.

397. The foregoing observations are farther applicable to those chequered or party-coloured leaves, many of which draw so much regard from their elegance and beauty. In many instances, these appearances proceed from age, from injury, or disease: but the regularity and uniformity of the colours, in other cases, indicate a natural state of the plant. In the *perfoliata pictis foliis*, says M. Senebier, the green leaves are spotted with yellow. The green leaves of the pimpernel of the mountain have yellow stripes. In a species of aloë, the middle part of the leaf is green, and the borders are yellow, while others are green and yellow, or yellow and green*. Now, in green plants, the yellow colour is formed by reducing the green, as in the leaves which fall in autumn, and this is effected by the development of acid matter. If, therefore, acid naturally prevail in one part of a leaf more than in another, a proportional degradation of colour will be produced, and this, too, in particular parts or places, according, probably, to variations in the structure of the leaf, or some other accidental circumstance; so as to give rise to that mixture of green and yellow which has been above stated. Should the acid abound still more, it may, as in certain red leaves of autumn, give rise to a red colour. Thus in the tri-coloured amaranth, the leaves are, at the same time, green, yellow, and red; but this is most frequent in autumn, as the power of vegetation declines †.

* Mem. Phys., tom. iii. p. 89. † Ibid. p. 93.

398. The red matter of these tri-coloured leaves is soluble in water, and is dissipated by light, leaving a yellow resinous matter, in which acid abounds *. It is also rendered green by alkalis †,—an observation which suggests the probability that its redness depends, as in other cases, on an excess of acid. Hence, then, the green colour of certain parts of these leaves would seem to depend on an excess of alkali, the yellow on a smaller portion of that substance, and the red on the predominance of acid matter:—all which colours are, in succession, exhibited by autumnal leaves, and by certain vegetable infusions, when submitted to the action of acid and alkaline matter. Of the truth of these opinions, an observation of M. Senebier affords additional evidence; for he remarked, that the tri-coloured leaves of the amaranth, exposed under water to the sun, afford air from their green parts alone, while the red and yellow portions yield no air ‡; so that, in the same leaf, we see the green colour accompany the decomposition of carbonic acid, and the yellow and red colours continue, when this acid is retained.

399. The general causes which thus give rise to the colour of leaves, seem, likewise, to act in the colouration of flowers. We have already noticed (25.) the agency of light in the production of these colours, by observing that roses, if kept in perfect darkness, are altogether deprived of colour. Blue hyacinths, says M. Senebier, become grey in dark-

* Mem. t. iii. p. 94. 95.

† Ibid. p. 94.

‡ Ibid. p. 99.

ness; poppies are white before their development; and carnations and roses do not acquire their rich hues but a short time before they are disclosed. Light, therefore, seems to act through the calyx of the flower in these instances; but the greater number of petals are green in the bud, before they are exposed to light. Others are white in the bud, and redden when they are disclosed*.

400. M. Becker made a great number of experiments on infusions of the flowers of different plants, by adding to them different acids, and various earthy, metallic, and alkaline salts. In general, the acids produced in these infusions a red colour, and the alkalis rendered them green, but sometimes yellow. In several instances, also, the nitric acid produced a yellow colour. All the acids reddened infusion of roses, but the alkalis and lime water rendered it yellow, without passing through green. The expressed juices of various flowers were likewise changed, from blue or violet to red, by acids; and brought back to their original colours by alkalis, which, if added in larger quantity, rendered them green or yellow. It results from these facts, that acids and alkalis rapidly change the colours of flowers; that acids, except the nitric, render them red, (and it also reddens the infusion of roses), while a green colour is constantly produced by the alkalis. In the change from blue to red by acids, the colour passes through different grades of purple; and the green colour, produced by the alkalis, passes ultimately to yellow,

* Mem. Phys. Chim. tom. iii.

which neither acids nor alkalis, nor even light itself, can change *.

401. Many experiments of a similar kind were made by Dr Lewis. The colour of many blue flowers, he observes, is extracted by water, but not by alcohol; and the expressed juice, also, of such flowers, is for the most part blue. Both the blue juice and infusion are reddened by acids, and rendered green by alkalis and lime water. The red colour, produced by acids, at length fades by exposure; and the green, produced by alkalis, changes to yellow. Red flowers impart their colour both to water and alcohol. The rose-coloured infusions were acted upon by acids nearly in the same manner as the blues; but the full deep reds were not. The deep infusion of red poppies was changed not to green, but to purple, by alkalis. Yellow flowers give out their colour to water and alcohol; but neither acids nor alkalis alter the species of the colour, though both of them vary its shade; acids rendering it paler and alkalis deeper. The fine yellow dust of the anthers of flowers gave a fine bright yellow to alcohol, and a duller yellow to water, which colours were heightened by alkalis, and turned red by acids. Even white flowers contain colourable matter, which is rendered green by alkalis, but not reddened by acids. The sulphurous acid vapour, which destroys the colour of other flowers, does not affect that of white ones †.

* Mem. Phys. tom. iii. p. 107.

† Neumann's Chem. by Lewis, p. 430. et seq.

402. Mr Delaval, also, digested red, purple, and blue flowers in acidulated water, and thus obtained red liquors, which, by very small portions of alkali, were changed to purple, blue and green. The addition of the alkali must, he says, be very gradual; for if too much be added at once, the intermediate colours between the red and the green will not be produced. He likewise digested red, purple, and blue flowers in alcohol, to which they yielded their colourable matter, and became white. From most of them, however, the alcohol acquired either no colour, or only a faint tinge; but when it was acidulated, it became red, and, by the addition of an alkali, assumed purple, blue and green colours*.

403. The foregoing facts clearly shew, that all flowers contain a colourable matter, which, whether it be extracted by water or by alcohol, experiences changes from acid and alkaline matter similar to those which the juices of the leaves exhibit, but modified, in some instances, by the peculiar qualities of the flowers employed. Hence we may venture to ascribe their colours to the diversified operation of the same general agents, which produce the colouration of leaves; and the infinite variety of tints which they afford, must, therefore, be referred to the various combinations of acid and alkali with their colourable matter, modified by the peculiar qualities of that matter, and probably by the structure of the

* Manch. Mem. vol. ii. p. 165.

part itself, as it may affect the reflexion or transmission of light.

404. That the red colours, at least of the damask rose, arise from the action of an acid substance, may be collected from some experiments of M. Senebier. He remarks, that the petals of this flower are rendered white by digestion in alcohol; but their former colour is restored if they be exposed to light and air. If, however, they be confined only in nitrogen gas, they do not recover their fine colour, but have only a yellow hue. The sun's light gradually accelerates the restoration of their colour, when they are confined in common air, but it is not indispensably necessary; for, where the air has free access, these petals resume their colour in obscurity, but more slowly than when exposed to light. When the colour is thus recovered, it is again given out to alcohol, and these operations may be repeated several times, until the colourable matter is exhausted. Various other flowers afford the same phenomena, but not in so marked a degree*. From these facts, we collect, that, though the sun accelerates the restoration of the colour, yet that air also is necessary; and since it is the oxygenous part of the air which alone seems to produce the effect, we may, from the analogy of similar changes on the colourable matter (372.) of plants, conclude, that carbonic acid is produced, and by its action restores the colour. This conclusion is supported by an observation of M. de la Folie, who remarks, that roses, which have been whitened by the vapour of burning

* Mem. Phys. Chim. tom. iii. p. 120. et seq.

sulphur, are reddened by acids, and rendered green by alkalis *.

405. M. Geoffroy, in his experiments on the oil of thyme, mixed with it different portions of acid and alkali, and obtained various tints of yellow, red, purple, violet, blue, green, and black. When the oil was dissolved in a considerable quantity of alcohol, it afforded a greyish colour, which became blue, by the addition of oil of tartar, and was rendered red by distilled vinegar, but recovered its blue tint, by a farther addition of alkali. In other instances, by the use of alkali, the colour passed to green; so that the oil of tartar, says M. Geoffroy, acts differently on the essential oil of thyme; for according as it is more or less concentrated, it renders it blue or green. As different colours are thus formed by the simple mixture of oils and salts, M. Geoffroy was led to suppose, that similar colours might be formed in plants by the same combinations. The principal colours, says he, which we observe in plants and in flowers, are green, lemon-yellow, orange-yellow, red, purple, violet, blue, black, and white; and of these colours, differently combined, all the others are composed. The blue, purple, violet, and green, he attributed to the operation of alkali; and the red and yellow, to the predominance of acid; the black he considered to be formed by the action of acid on a purple; and the white to be produced by a very copious reflection of light, from transparent and colourless particles †.

* Mem. Phys. Chim. tom. iii. p. 140.

† Mem. de l'Acad. Roy. an. 1707.

406. This enumeration of the principal, or fundamental colours of vegetables, will recall to the mind of many readers, the system of colours, which the celebrated Werner has contrived, and applied to the description of the bodies which compose the mineral kingdom. The colours of minerals yield, perhaps, neither in number, in diversity, nor in splendour, to those which vegetables present; yet they are all reduced to eight, which are regarded as standard or simple colours. These colours are white, grey, black, blue, green, yellow, red, and brown. Although several of these are physically compound, yet, for the purposes of description, it is convenient to regard each as simple, and as constituting, in its pure and unmixed state, what may be denominated the *characteristic colour*. To these eight fundamental colours, Werner refers all the variety of compound colours which minerals present, employing the predominating colour to express the chief character, and qualifying it by the others, according to the quantity in which they appear to enter into the compound *. If, from the enumeration of Geoffroy, we reject one of the yellows as superfluous, his number of fundamental colours will agree with that of Werner; and even their characters will differ only in two instances, M. Geoffroy considering violet as a characteristic, or simple colour, which Werner describes as a compound of blue, red, and brown; and the latter

* Jameson's Treatise on the Ext. Characters of Minerals, p. 2.

has no colour answering exactly to the purple of Geoffroy.

407. It is, however, our object rather to investigate the general causes which give rise to colour in vegetables, than to detail and describe their particular varieties and hues. But if the colourable matter of vegetables were extracted in various ways, and submitted to the action of acid and alkaline agents, of known strength, and in various proportions, not only might a vast variety of tints be produced, but it would not be difficult to determine, by calculation, the relative proportions in which those agents, in different instances, contributed to their formation. And were the fundamental colours properly defined, and their grades and varieties arranged, and classified according to the method of Werner, we might perhaps obtain not only a regular series or suite of colours, answering, in some degree, to the diversity found in nature, but might, also, arrive at comparative estimates of the proportions of acid and alkaline matter, by which they were respectively produced.

408. Lastly, the colours of fruit experience changes similar, in many respects, to those of leaves and flowers; and, apparently, from the varied operation of the same general causes. In their earliest state, many fruits are green; but in the more advanced periods of growth, they assume different tints of colour. To the production of these colours, however, the light of the sun seems to be necessary; for, according to M. Senebier, neither peaches, pears, nor cherries, acquire their proper lively colours, if, at the period of ripening, they are secluded from the action

of light. Those leaves also, which intercept the sun's light, delineate on the fruit beneath the bounds which they prescribe to its action; and if a portion of fruit be covered with a piece of tinfoil, the uncovered portion will become perfectly red, while the covered part will exhibit only a pale or straw colour. So, likewise, if grapes, which would have become violet by exposure, be inclosed in black paper or glass, which excludes the light, they assume only a grey colour. Those green fruits which do not redden, lose also their green colour in ripening, and become yellow. This change is quickened by the action of light, but it also takes place without it; for many fruits pass from green to yellow, although they are secluded from light*.

409. The red juices of many fruits are extracted by water and alcohol; and both the solutions, and the juices themselves, says Dr Lewis, are sometimes made more florid by acids, and generally turned purple by alkalis†. The skins of fruits, likewise, yield their colourable matter to water and alcohol. The red skin of the peach imparts a red tinge to water and alcohol, but the white skin of the same fruit renders alcohol green. When the red skin has been thus deprived of its colour, it again assumes it, like the petals of roses, by exposure to light and air. The skins of different green, red, and yellow fruits imparted their respective tints to alcohol, and some, in a small degree, to water‡; and the red tinctures of these

* Mem. Phys. Chim. t. iii. p. 146. 7.

† Newmann's Chemistry, p. 432.

‡ Mem. Phys. Chim. tom. iii. p. 148. 9.

fruits were exalted by acids, and changed to purple or green by the addition of alkalis *. The foregoing facts sufficiently prove, that the colourable matter of fruits, like that of flowers, and of leaves, is extracted by the same means, and acted upon, in a similar manner, by the same chemical agents; wherefore we are entitled to conclude, that its nature is essentially the same, and that it exhibits different appearances and colours, according to the modified action of those causes which produce the colouration of all the other parts of the plant.

410. The process of maturation, during which the changes of colour in fruits principally take place, is known to be accompanied by spontaneous changes, under which acid matter is developed, and produces effects on the colour of fruits not unlike those which decomposition occasions in the leaves, at the period of their fall. Whatever be the nature of the acid developed, its power in producing changes in the colourable matter will, probably, be nearly the same; and the existence of such acid is often sensible to the taste, though disguised frequently by the presence of saccharine matter. It is a farther confirmation of these views, that green fruits, like other green parts of plants, were found by M. de Saussure (274.) to afford oxygen gas in sunshine; which fact evinces, that they not only previously contained carbonic acid, but that, as in other cases, they became green, when this acid was decomposed and expelled.

* Mem. Phys. Chim. tom. iii. p. 153.

SECT. IV.—Of the Physical and Chemical Agency of Light,
in promoting the Colouration of Plants.

411. So far we have spoken of the colours of plants, as arising from variations in their chemical constitution alone ; and these variations we have attributed to the decomposition of their saline compounds, by the agency of solar light. We have next to inquire into the manner in which light exerts this action, a subject of very nice and difficult investigation, but the importance of which will, we hope, furnish an apology for the apparent temerity of our attempt, even although we should fail in its execution.

412. Besides this chemical action, which light has been shewn to exert, we have also to consider its property of imparting colour ; for when it has been said that the predominance of acid or alkaline matter renders the vegetable juices red or green, it must be understood to mean only, that such a state or constitution of those juices is thereby induced, as, in the language of Sir Isaac Newton, enables them, more or less, to reflect or transmit the red, or green-making rays of light. We do not, however, propose to go far into the consideration of these subjects, but to select and exhibit such facts only, as force themselves on our notice, by the near connection which they seem to have with the more immediate objects of our research.

413. Since the great æra in science, created by the genius of Newton, who first decomposed the solar beam, and thus

“Untwisted all the shining robe of day,”

it has been considered, that “colours are not qualifications of light, derived from refractions or reflexions of natural bodies (as was generally believed), but original and connate properties, which, in divers rays, exhibit divers colours*.” By means of the prism, Sir Isaac Newton was enabled to separate the solar beam into seven differently coloured rays, which run gradually into each other according to their particular degrees of refrangibility. Of these rays, the violet is the most refrangible; next the indigo; then follow the blue, the green, the yellow, the orange, and the red, which is the least refrangible of all. In the property of reflexibility, these rays, also, follow the order of their refrangibility.

414. These primary rays of light are simple and homogeneous, and cannot be changed in colour either by reflexion or refraction. Different mixtures of them may, however, be made to compound colours, like to those of homogeneous light; and various other colours, unlike to any of those of the primary rays, may be also formed by composition. But the red, yellow and blue rays are incapable of being produced, like all the rest, by the combination of other colours; they are therefore always simple and uncompounded. When all the primary rays are mixed together in due proportion, they then constitute white light.

415. Having thus examined the several properties of the primary rays, this great philosopher proceeded to apply his discoveries to the explanation of the permanent colours of natural bodies. These colours, says he, arise from hence ; that some bodies reflect some sorts of rays ; others, other sorts more copiously than the rest. Every body reflects the rays of its own colour most copiously, and from the excess or predominance of these rays in the reflected light derives its colour. And while bodies become coloured by thus reflecting this or that sort of rays, it is to be considered, he adds, that they stop and stifle in themselves those rays which they do not reflect *. Hence it appears that Newton considered all coloured matter to reflect light ; and this reflexion he supposed to be made by some power of the body which is evenly diffused all over its surface, and by which it acts on the body without immediate contact †.

416. According to Dr Wells, however, both Kepler and Zucchius had previously shewn, by experiment, that light is reflected without colour from the surfaces of bodies ; and that the colours of bodies depend, therefore, not on the light reflected by their anterior surfaces, but upon that portion which has entered their internal parts, and is from thence sent back through those surfaces ‡.

417. Beside the colours which are thus exhibited by reflexion, bodies appear coloured by the light that is transmitted through them ; and the bodies which thus appear of any colour by transmitted light, may,

* Optics B. i. prop. 10. prob. 5. — † Ibid. B. ii. prop. 8.

‡ Phil. Transact, an. 1797, p. 418.

also, says Newton, look of the same colour by reflected light *. But the numerous experiments of Mr Delaval prove, that, in transparently coloured liquors, the colouring matter does not reflect any light; and that, if the light, which such liquors transmitted, be stopped, they do not vary from their former colour to any other colour, but become entirely black †. This conclusion he extends to transparently coloured solids as well as fluids ‡. The light, also, which opaquely coloured bodies, as plants, return to the eye, continues Mr Delaval, is reflected by their white opaque substance; and the colours of vegetables, therefore, are produced by the light reflected from this white matter, and transmitted from thence through the coloured coat, or covering, which is formed on its surface by the colouring particles §.

418. Admitting, then, that the permanent colours of natural bodies depend on the varied reflexion or transmission of the differently coloured rays, we have yet to learn why these bodies reflect or transmit some sorts of rays in preference to others, so as to present to our view that great diversity of colours which they exhibit. Sir Isaac Newton resolves this property of bodies into the varying thickness or density of their particles, and lays it down as a general law, that the forces of bodies to refract and reflect light are very nearly proportional to their densities; so that nothing more, says he, is requisite for producing all the co-

* Optics B. i. part 2. prop. 10.

† Manch. Mem. vol. ii. p. 146.

‡ Ibid. p. 159. § Ibid. p. 192.

lours of natural bodies than the several sizes or densities of their transparent particles *.

419. These conclusions he drew from observing the colours afforded by plates of air and water, submitted to compression between two object glasses, whereby various colours were made to emerge, in successive orders, according to the degree of pressure employed. Extending the same principles to explain the permanent colours of natural bodies, he ascribed the yellow, orange, and red colours which certain liquors exhibit at different depths, when viewed in a conical glass by transmitted light, to the varying thickness or density of such fluids, which successively intercept and extinguish the more refrangible rays, until, near the top, they transmit the least refrangible, or red rays alone †. In like manner, he supposed vegetable infusions to be turned red by acids, because it is the nature of acids to dissolve or attenuate; and that the same infusions were rendered green by alkalis, because these substances precipitate or incrassate ‡. Mr Delaval follows Newton in attributing these phenomena to the same causes; and more lately, M. Haüy observes, that, in all these changes of colour the union of the *moleculæ* of the two liquids forms mixed *moleculæ*, the thickness of which is different from that of the component *moleculæ*, and determines the reflexion of colour answering to that thickness §.

* Optics, B. ii. part 3. prop. 10.

† Ibid. B. i. part 2. prop. 10.

‡ Ibid. B. ii. part 3. prop. 7.

§ Elem. Nat. Phil. vol. ii. p. 243.

420. This hypothesis, founded solely on the mechanical constitution of bodies, has, however, been warmly impugned by Dr Bancroft, who contends that the phenomena, exhibited by the coloured rings in Newton's experiments, do not warrant the conclusions drawn from them; for that the same colours occurred, and were repeated over and over again, at very great diversities of thickness; so that thickness could not be the only cause of these repeated variations of colour. And, indeed, they are to be explained, he adds, in the same manner as the colours of the prism and the rainbow, and ought not to have been employed to explain the permanent colours in different substances*.

421. On Mr Delaval's attempts, to shew that the different colours of animal and vegetable substances depend on an increase or diminution in the size of their particles, Dr Bancroft remarks, that, instead of employing mechanical means, which alone ought to have been used in his experiments, he has recourse to chemical agents, which change the composition of bodies, and produce effects different from those which arise from mere variation of density†. And in some examples given by Dr Bancroft, so far are the refractive and reflective powers of bodies from being in proportion to their density, that they are observed to be rather in opposition to it‡.

* On Permanent Colours, p. 7.

† Ibid. p. 19.

‡ Ibid. p. 23.

422. But though the Newtonian hypothesis appears thus to fail in its general application to the permanent colours of natural bodies, yet, in the examples of liquors which possess an uniform composition, and which, at different thicknesses, exhibit different colours, when viewed by transmitted light, it may, perhaps, to a certain extent, be deemed satisfactory; for as all bodies intercept a portion of the light that falls on them, it is reasonable to suppose, that the increasing mass or density of the fluid should furnish an increasing obstacle to the passage of light, until, at the thickest part of the liquor, the least refrangible, or red-making rays alone are transmitted; and such seems to have been truly the cause of the redness which the upper portion of the liquor presented in Newton's experiment (419.), and which Dr Halley's hand exhibited at great depths in the sea *. The red colour of the sun too, in certain states of the atmosphere, seems to arise from a similar extinction of the more refrangible rays; and so, likewise, the light from our lamps, in very hazy weather, is not only diminished in intensity, but varied in species, so as to approach nearly to perfect redness.

423. But when vegetable infusions are changed in colour by acids and alkalis, a great discordance seems to obtain between the imputed properties of those substances, and the established qualities of the several rays of light. For if acids do dissolve and attenuate (419.), they must, on the mechanical hypothesis, be considered to facilitate the passage of light; and yet, after the addition of an acid, the more re-

* Optics B. i. part 2. prop. 10.

frangible rays are at once stopped, and the red-making rays, which are the least refrangible, are those only which pass; while, on the contrary, alkalis, which are supposed to precipitate and incrassate, and consequently to increase the resistance to the transmission of light, stop suddenly the red rays, and permit the green, and even the violet, the most refrangible, and therefore the most easily intercepted of all the rays, to pass;—effects, in each instance, the reverse of those which these agents ought to produce. The small portion, also, of acid or alkaline matter, that is required to change the colour of an infusion, can hardly be supposed to vary the density of the mixture so much as, by that mean alone, to produce such great alterations in its appearance: and this is still more difficult to conceive, when the addition is made, as it often is, by a body in a gaseous form. Lastly, the constitution of the fluid has itself undergone a change, not merely from a change of density, but of composition also, which obliges us to take into account the chemical, as well as the mechanical effect.

424. An observation, also, of Mr Delaval, points out a great dissimilarity in the action of different liquors upon the rays of light. In Newton's experiment (419.), says he, it is probable that the liquor employed was an aqueous or spirituous infusion of the woods used in dyeing red, which transmits yellow, orange, or red colours, according to its thickness; but the red solutions of flowers, and many others, do not transmit yellow or orange colours, even when spread thin; for when thus disposed in thicker or

thinner masses, they do not vary the *species* of their colour, but only transmit a diluter, or more intense red *. We may therefore venture to conclude, that the opinion of Newton, respecting the colours produced in vegetable infusions by acids and alkalis, was derived, merely by analogy, from what he had observed to happen in liquors apparently of the same nature, but which act in a very different manner on light; and this difference, we may add, furnishes a new proof that chemical composition, not less than mechanical constitution, influences the permanent colours of natural bodies.

425. Sir Isaac Newton himself, indeed, may be considered as admitting the influence of chemical composition in varying the action of bodies upon light; for, in stating his general law, he makes a well-known exception with respect to inflammable bodies, which, according to him, refract light more powerfully than other bodies of the same density. So much stress, indeed, did he lay on the great refractive power of these bodies, that he even thought it “rational to attribute the refractive power of all bodies, chiefly, if not wholly, to the sulphurous (*i. e.* the inflammable) parts with which they abound; for, adds he, it is probable that all bodies abound more or less with sulphurs †.” “And this great man,” says Dr Bancroft, “having also concluded that the permanent colours of natural bodies were analogous to those produced by the refractions of thin, colourless,

* Manch. Mem. vol. ii. p. 235.

† Optics, P. ii. part 3. prop. 10.

transparent plates, the chemists were generally induced to make all colour depend on the *principle of inflammability* or *phlogiston*, which, until very lately, was supposed to exist in metals and other substances, where there certainly is no evidence or appearance of it."

426. "But since the existence of phlogiston in bodies has been denied by the pneumatic chemists," continues Dr Bancroft, "they have, in most cases, attributed the origin and changes of colours to the application or combination of different gases, and particularly of *oxygen*, in different proportions; and it has been supposed that these gases possessed considerable refractive powers, and were thereby enabled to produce effects on colours, like those which the followers of Stahl had imputed to phlogiston*." "Hence," he adds, "M. Berthollet, in his work on dyeing †, intimates that many important observations remain for those who would follow the steps of the great Newton, and compare the *refractive* powers of the different gases, and of other substances, the constituent principles of which are now known."

427. This opinion concerning the influence of oxygen, in producing the colouration of bodies, seems to have arisen from the well-known changes of colour which certain metals exhibit during their oxidation. If, however, oxygen were the cause of these changes of colour, some uniformity of effect might be expected to attend its different degrees of combina-

* Philos. of Perm. Colours, p. 27.

† Elem. de la Teinture, tom. i. p. 5.

tion ; but it is well known that metallic oxides exhibit every variety of colour, with little or no regard to the quantity of oxygen they contain. Thus, many oxides, which contain but little oxygen, are white ; others, which contain a great deal, are black. Mercury, with different portions of oxygen, forms a black and red oxide, but these colours are not peculiar to such combinations ; for sulphur, in union with mercury, produces compounds, which exhibit similar colours. Iron, when combined with 27 *per cent.* of oxygen, is black ; with 48 *per cent.* it is red ; and these same combinations exhibit sometimes green and brown colours. So many, indeed, and so diversified are the colours which the oxides of iron are capable of assuming, that all the fine varieties of colours, employed in Wedgwood's pottery, are said to be produced by the oxides of this single metal *. Hence, then, it appears, that, although oxygen combines with metals in various proportions, and the compounds exhibit various colours, yet no uniform colour, even in the same metal, accompanies the varying proportion of oxygen. On the contrary, similar colours are produced by very different combinations of this element ; and different substances, by uniting with metals, exhibit similar colours where no oxygen is concerned. Consequently, no particular colouring property belongs exclusively to oxygen, and, therefore, the colours of metallic oxides must be attributed to the properties of the compound, and not exclusively to those of the oxygen they may contain.

* Bancroft on Perm. Colours, p. 17.

428. Neither does the supposed refractive power of oxygen give any support to this doctrine; for, from the experiments of M. M. Biot and Arrago, it appears, that hydrogen, in this respect, exerts the greatest force; and oxygen, on the contrary, is one of the bodies that has the least refractive power. Hence it is from the hydrogen they contain, that water, oil, and other substances, refract light in a ratio so much surpassing their density *. These results, therefore, accord perfectly with the facts observed, and the conjectures submitted by Newton, respecting the great refractive power of inflammable bodies; but they yield no support to the opinion entertained of the predominance of this property in oxygen.

429. With respect, also, to vegetable bodies, it is not easy to conceive how the presence of oxygen should produce their various colours. Except the pure bases of the alkalis, the earths, and the metals, almost every substance in nature appears, from Mr Davy's researches, to contain a small portion of oxygen; so that, in every change of composition which bodies undergo, oxygen must be more or less engaged. The universality of its presence, however, taken in connection with the ever-varying proportions in which it exists in bodies, and the infinite variety of shades and colours which these bodies present, furnishes arguments against the idea of its acting as the colouring principle of matter: nor does it readily appear why the oxygen, more than any other ingredient of the body, should give rise to the formation

* Haüy's *Traité de Phys.* vol. ii. p. 185.

of its colour. Because air is necessary to the vegetation of plants, M. Fourcroy, indeed, ascribed the different shades of vegetable colours to the different degrees in which he supposed oxygen to combine with them^{*}; but it has been sufficiently shewn, that, during vegetation, the oxygenous portion of the air never combines with plants; and that, as far as external agents are concerned, light, and not air, is the ostensible cause of their colouration. The ordinary effects of acids in changing colours, must not be attributed to the oxygen they contain, but to their action as acids; for muriatic acid, which does not yield its oxygen to any known substance, acts, like other acids, upon vegetable infusions; and oxy-muriatic acid, which, of all substances, most readily parts with its oxygen, entirely destroys all vegetable colours†. In whatever light, therefore, this subject is viewed, no good reason appears for considering the element of oxygen as essentially concerned in producing the colouration of natural bodies.

430. Since, then, neither the mechanical doctrine of density, nor the supposition of phlogiston, nor the actual combination of oxygen with bodies, seems sufficient to explain those affections of light, from which the diversity of their permanent colours proceeds, we must seek out some other mode of ac-

^{*} Mem. de l'Acad. an. 1789, p. 335.

† In our opinion, the late experiments of Mr Murray seem to establish the compound nature of oxy-muriatic gas, in opposition to the view of Mr Davy, who regards it as a simple substance^{*}.

^{*} Nicholson's Journal, Feb. 1811.

counting for these phenomena, less liable to objection than any that has been hitherto proposed. It is remarked by Dr Bancroft, that, though the prism and other transparent colourless substances shew us the different colours of the several rays of light, by separating them from each other, in consequence of their greater or less refrangibility; yet he is persuaded that the *permanent colours* of different bodies, or substances, *are not produced by mere refraction*; but depend on other properties, which determine or occasion the reflection or transmission of some particular sort or sorts of rays, and an absorption or disappearance of the rest: and these properties he conceives to be certain *affinities* or *elective attractions*, existing in, or between the differently coloured matters, and the particular sorts or rays of light so absorbed or made latent*.

431. That an affinity, or attraction, is exerted between light and the particles of bodies, may be justly inferred from the great refractive power of inflammable bodies, which, all other things being equal, must be supposed to attract light more powerfully than other substances; and it is *variation* in point of strength, says Dr Thomson, which constitutes the characteristic mark of chemical affinity†. The phenomena of phosphorescence, and many other chemical facts, afford evidence of the same nature. Thus, many metallic oxides, as Scheele first observed, are soon restored to their metallic form, by the action of

* On Perm. Colours, p. 29.

† Syst. Chem. vol. i. p. 246. 1st edit.

light *. The muriate of silver, when exposed to the solar rays, begins to be discoloured at the end of a few seconds ; after a minute, says M. Senebier, its surface is sensibly violet ; and in half an hour, the violet is changed to the shade of umber, and then suffers no farther change. This change is effected entirely by light alone ; for when this muriate is exposed to heat, or cold, or moisture, in a dry air, or *in vacuo*, it suffers no change, if the light be carefully excluded. If, however, the light be thrown on it by a lens, it is then coloured in an instant. If it be covered by one leaf of paper, the discolouration does not begin till the expiration of a minute ; if with two leaves, three minutes are required ; if with three leaves, ten minutes ; and four leaves entirely prevent the action of light †.

432. In effecting this discolouration, it was remarked by Scheele, that the violet ray acted sooner than any other ‡ ; and Senebier having thrown the prismatic rays, in succession, on portions of this muriate, observed that the violet ray acted in 15 seconds ; the indigo in 23 seconds ; the blue in 29 ; the green in 37 ; the yellow in five minutes and a half ; the orange in twelve minutes ; and the red in twenty minutes ; but the three last species never produced the effect so strongly as the others §.

* On Air and Fire, p. 78. et seq.

† Mem. Phys. Chim. t. iii. p. 199.

‡ On Air and Fire, p. 91.

§ Mem. Phys. Chim. tom. iii. p. 199.

433. The muriate of silver, which is thus acted on by light, is composed of 75 parts oxide of silver, 18 muriatic acid, and seven of water. Its discolouration has been ascribed to a partial reduction of the oxide ; but, by operating upon the salt under water, Scheele and Berthollet found, that muriatic acid was liberated ; and to the disengagement of this acid alone, the discolouration seems to be owing *. In conformity with this explanation, Dr Bancroft observes, that, if this salt be covered with muriatic acid, instead of water, it experiences no change, though exposed, for many days, to the direct rays of the sun †.

434. The action of light on vegetables, seems to resemble, in many respects, its operation on inanimate bodies. We have already seen, that light, in its undecomposed state, causes the expulsion of oxygen gas, and gives rise, at the same time, to the green colour of plants. To discover the manner in which it was more immediately concerned in these operations, M. Senebier sowed different quantities of lettuce seeds in several small cups. One of these cups he left exposed to the light and air ; another he placed in darkness ; a third he confined under a large glass vessel, whose bottom was thrust so far up into its body, as to leave a hollow space, nine or ten inches in height, and four or five in width ; this vessel was then filled with water, through which the light, that fell on the seeds beneath, necessarily pass-

* Murray's System of Chemistry, vol. iii. p. 123.

† On Permanent Colours, p. 35.

ed ; a fourth cup was placed under a similar vessel that contained a yellow fluid ; a fifth, beneath a similar vessel filled with a red fluid ; and a sixth, under one that contained a fluid of a violet hue ; so that through these fluids, the yellow, red, and violet rays were respectively transmitted, while the others were, for the most part, intercepted.

435. Observing, then, the effects produced by the different portions of light, which were thus permitted to act, he found that the plants, illuminated by the yellow rays, grew most rapidly in height ; next, those in the violet rays ; afterwards, those in the red rays. The plants which grew in light, transmitted through water, were still smaller, and approached in size to those which flourished in the open air ; while those in perfect darkness attained the greatest height of all. These last plants perished on the eighth day, and those in the yellow light on the ninth day ; while all the others continued to vegetate. At the end of about five weeks, the plants, growing under the red vessel, were four inches and nine lines in height ; under the violet vessel, three inches and three lines ; under the water vessel, two inches and ten lines ; and one inch and three lines in the open air.

436. With respect to the general appearance of the plants, the leaves of those which grew in red light were smaller and less smooth, than those of the plants in violet light ; or than the leaves of the plants confined under water, or than the leaves of those which grew in the open air. As to colour, the leaves exposed to yellow light were at first green,

but afterwards became yellow; those in red light appeared green, and preserved a tinge of that colour; those in violet light were quite green, and their colour augmented with their age; while those raised in obscurity possessed no verdure at all *.

437. These experiments were repeated on French beans, and with results nearly similar. In proportion as the plants grew in height, in different kinds of light, the number and size of their leaves diminished. In the free air and light, the leaves of beans, shooting out of the earth, became green in a day or two, according to the intensity of the light; those leaves which received light through water, had a deeper green colour; those in violet light acquired a deep green, approaching to blackness; those growing in red light were also green, but less so than natural leaves. In all these experiments, more or less light was reflected by the fluids employed, or intercepted in its transmission through them. If, therefore, the effects produced come near to those which attend the action of entire light, it must, says M. Senebier, be owing less to the intensity of illumination, than to the quality of the illuminating ray. Hence, when the violet ray renders plants as green at least as entire light, this cannot arise from its power of illumination; for this ray is only a part of entire light, and is, besides, transmitted through a glass vessel, and through the fluid which that vessel contains. But the violet light, which thus acts on the colour, does not equally act on the growth and development of the plant; whence it is concluded, by M. Senebier,

* Mem. Phys. Chim, t. ii. p. 55. et seq.

that the height and size of the plant are proportional to the intensity of illumination, while its verdure depends more on the quality of the ray *.

438. The above experiments, not only prove the action of light in the colouration of plants, but demonstrate, likewise, that this action is exerted most powerfully by the violet ray. They shew, also, that this property of the violet ray is independent of its *illuminating* power; but they do not authorise the conclusion, that the height and size of the plant depend on the intensity of illumination, for we know that other agents contribute to these conditions. By other experiments, M. Senebier also ascertained, that the *heating* power of the violet ray had no particular influence in effecting these changes of colour; for, by placing thermometers in the differently coloured rays of the prismatic spectrum, he found that the violet ray possessed less heat than the others, and less, also, than the entire beam of light; whence, says he, it results, that these properties of the violet ray are independent, in a certain degree, both of its heating and illuminating power, and appear to depend on some particular quality of the matter of which it is composed †.

439. M. Berthollet, likewise, who, as we have seen (362.), considers the action of light, in discharging colours, to resemble combustion, and thus to cause the combination of oxygen with bodies, admits, nevertheless, many facts which present an apparent contradiction to it. It is to the action of the

* Mem. Phys. tom. ii. p. 62.

† Ibid. p. 72.

sun's rays, says he, that the production of vegetable colours is owing. Light, also, disengages oxygen from nitric and oxy-muriatic acids, from some metallic oxides, and from plants in a state of vegetation. In these examples, he continues, effects opposite to those of combustion are produced ; but when it contributes to the destruction of colours, it combines oxygen, and produces a kind of combustion. What, however, are the circumstances, and what the affinities, which determine sometimes one effect, and sometimes the other, he does not know ; but both, he adds, are equally proven *. Dr Bancroft, also, refers to different instances, in which light acts sometimes by separating, and sometimes by combining, oxygen with bodies ; which he ascribes to the varied operation of an affinity exerted between them †. These various, and apparently contradictory effects of light, may, perhaps, receive illustration from a more minute inquiry into the nature and constitution of that subtle matter. The researches of modern philosophers furnish many new and important facts to aid our investigation ; and it is by collecting and comparing these that we are led to indulge the hope of being able to penetrate somewhat farther into the secrets of its chemical action.

440. THE colours of the primary rays of light, and their different degrees of refrangibility and re-

* Elem. de la Teinture, tom. i. p. 58.

† On Permanent Colours, p. 46.

flexibility, were, as we have seen, discovered by Newton; and the same incomparable philosopher pointed out also the difference in their *illuminating* power. The most luminous of the prismatic rays, says he, are the yellow and orange; next to these, the red and green; the blue is fainter; and the indigo and violet are still more dark and faint. The most luminous and fulgent part is in the brightest yellow, where it inclines more to the orange than to the green *. Dr Herschell, by causing the prismatic rays to fall successively on an object placed beneath a lens, found the illuminating power to be small in the red ray, greater in the orange, and greatest in the centre of the spectrum, between the yellow and green. From the full deep green, the illuminating power decreased very sensibly, being in the blue nearly on a par with that of the red, and in the violet it was least of all. By other experiments, he ascertained that this property resides in peculiar rays, which are distinct from those that impart heat; for the illuminating rays are capable of being transmitted through substances by which the heating rays are stopped, and *vice versa*;—facts which the experiments of Scheele had before shewn to be the case with the heat and light which radiate from a common fire †. To these facts we may add, that the light of the moon, though composed of all the prismatic rays, does not, even when concentrated by a lens, impart the least sensible heat. Hence then the rays, which render objects

* Optics, B. i. prop. 7.

† On Air and Fire, p. 70.

visible, and enable them to exhibit colour, possess peculiar properties, and are entirely distinct from those which excite or produce heat.

441. The heating or *calorific* rays, which enter into the composition of solar light, appear to have been first distinguished as a distinct species of matter, by M. Rochon, who, in the year 1775, discovered that the rays, which differ in refrangibility, differ also in their power of heating bodies. He observed that an air thermometer, moved through the prismatic spectrum, rose in proportion as the rays followed one another from the violet to the red extremity; so that the heat between clear red and the most intense violet appeared nearly as eight to one*. M. Senebier also, (438.), by a similar mode of experiment, ascertained the same fact with less precision; and Dr Herschell, in a similar manner, calculated the difference nearly as seven to two. Pursuing the subject still farther, Dr Herschell ascertained that this calorific effect was produced by rays, different from those which impart light and colour; that it was, in fact, produced by *invisible* rays, which increase progressively in power from the violet to the red extremity of the spectrum, and exert the greatest effect about half an inch beyond the boundary of the latter†. These facts are farther confirmed by Sir H. Englefield, who having successively collected the prismatic rays into the focus of a lens, found the violet, in three minutes, to raise the thermome-

* Haüy's Nat. Phil. vol. ii. p. 256.

† Phil. Trans. 1800.

ter, placed in its focus, only one degree; while the red rays, in two minutes and a half, raised it sixteen degrees; and when the thermometer was carried quite out of visible light, it rose, in two minutes and a half, eighteen degrees *. From these experiments we learn, that the *heating* power of solar light consists in *invisible* rays, which are entirely distinct from those which produce illumination and colour. It may be added, that Scheele had previously ascertained, that the caloric, which radiates from a common fire, and causes ignition, consists, likewise, in *invisible* rays †.

442. But we have also seen (432.), that the *chemical* action of light is most powerfully exerted by the violet rays, which are far removed from the centre of the spectrum, where the illuminating power is greatest, and still farther from its red extremity, where the calorific effect is most intense. As, therefore, in their refrangibility, they seem to follow laws so distinct from the two other species, it was natural to expect that they should, also, consist of a distinct kind, or third species of matter. This, M. Ritter has discovered to be actually the case, and that, like the calorific rays, these chemical rays are *invisible*, and possess the greatest power beyond the violet boundary of the spectrum. He found that muriate of silver, which became black when placed beyond the confines of the violet ray, gradually lost its dark tint, as it was moved through the other rays towards the red extremity of the spectrum; and when the

* Murray's Syst. Chem. vol. i. p. 519. † On Air and Fire, p. 76.

same substance, a little blackened, was exposed to red light; it recovered in part its whiteness, especially when presented to the invisible calorific rays, which lie beyond the limit of the red extremity. He observed, also, that phosphorus, when placed near the red rays, instantly exhibited white vapours; but, when moved into the violet rays, these vapours no longer appeared, and its combustion was extinguished. Hence he was led to conclude, that the solar spectrum was comprised between two sets of *invisible* rays, which produce the opposite effects of combining and separating oxygen from bodies, while the intermediate parts of the spectrum partake more or less of the action that is exerted by the invisible rays on either side *. These chemical rays were, likewise, discovered by Dr Wollaston, who found that muriate of silver was blackened, not only in the space occupied by the violet ray, but in an equal degree, and to about an equal distance, beyond the visible spectrum; and, by narrowing the pencil of light received on the prism, the discolouring rays were made to fall almost entirely beyond the violet: whence he inferred, that visible light does not possess this discolouring property, but owes its influence, in this respect, to the admixture of invisible light †.

443. From the foregoing series of facts, it would appear, that solar light is made up of three distinct species of rays; one species of which produces heat, and promotes the combination of oxygen with

* Haüy, vol. ii. p. 259.

† Phil. Trans. 1802, p. 379.

bodies : a second species is luminous, and imparts colour to objects ; and the third species exerts a chemical action on bodies, and causes the separation of oxygen from them. It likewise appears, that, while the luminous rays differ gradually from each other in their degrees of refrangibility, the calorific and chemical rays are, in this respect, entirely opposed, and are therefore found, in greatest intensity, near to and beyond the opposite boundaries of the prismatic spectrum.

444. The different chemical effects, produced in bodies by the agency of light, which M. Berthollet so strongly remarked (439.), but was unable to explain, receive, we think, an easy solution from these views of the different nature and operation of its calorific and chemical rays ; for these two portions of light seem respectively fitted to produce the opposite effects of combination and decomposition, which he has noticed. These effects, M. Ritter has farther remarked, to resemble those which are produced by the opposite electricities of the Voltaic pile ; for, while positive electricity, in the decomposition of water, occasions, like the calorific rays, the combination of oxygen with the metallic wire, no such effect takes place at the opposite, or negatively electrified wire *. To follow this analogy with greater precision and effect, let us consider, somewhat more particularly, the chemical operation of Galvanic electricity, as it has been developed in the late important experiments of Mr Davy.

* Haüy, vol. ii.

445. It appears, from the investigations of this distinguished chemist, that the elements of bodies are not only separated from each other by the operation of Galvanic action, but are actually transferred to distant places, in a state or condition which, for a time, entirely suspends the exertion of their chemical powers. Thus, if two portions of a neutral salt, as muriate of soda, be exposed, in separate glasses, to the respective poles of the Voltaic battery, and the circuit be completed by a moistened substance, plunged on each side into an intermediate vessel that contains pure distilled water, the salt in each glass undergoes decomposition. At the positive pole, the acid is attracted, and the alkali is repelled : at the negative pole, the reverse operations take place ; while the repelled alkali from the one side, and the repelled acid from the other, meet in the middle vessel, and recombine muriate of soda. “ So from the general phenomena of decomposition, and transfer, it is easy,” says Mr Davy, “ to explain the mode in which oxygen and hydrogen are separately evolved from water. The oxygen of a portion of water is attracted by the positive surface, at the same time that the hydrogen is repelled by it ; and the opposite process takes place at the negative surface ; and in the middle, or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only be active, there must be a new combination of the repelled matter *.”

* Phil. Trans. 1807.

446. "These facts," continues Mr Davy, "seem fully to invalidate the conjectures of M. Ritter, with regard to the elementary nature of water. He conceived that he had procured oxygen from water without hydrogen, by making sulphuric acid the medium of communication at the negative surface; but, in this case, sulphur is deposited, and the oxygen from the acid, and the hydrogen from the water, are respectively repelled, and a new combination produced *." In support, however, of his opinion, M. Ritter has since urged the result of another experiment, to which many eminent chemists have yielded their assent. He decomposed water in two separate glasses, connected with each other by a metallic arc, and obtained oxygen at the positive, and hydrogen at the negative pole, in the usual manner. Deeming it impossible, however, that the two corresponding elements, which were extricated, could be repelled through the substance of the metallic arc, he was induced to deny altogether the decomposition of the water, and to maintain its elementary nature, conceiving that, by its combination with positive electricity, it formed oxygen gas, and, by its union with negative electricity, it constituted hydrogen gas.

447. The fallacy of the experiment, which led to this conclusion, has, however, been clearly detected by Mr Murray. He formed the Galvanic arrangement employed by Ritter, and observed oxygen and hydrogen to be liberated in the respective glasses; but, on a closer examination, he discovered that the

* Phil. Trans. 1807.

two extremities of the metallic arc soon acquired, by the law of *induction*, electricities the opposite to those possessed by the Galvanic wires. Consequently, the effects of a double battery were obtained, and, in each glass, two particles of water were decomposed, just as happens in one glass, when the two wires of the battery approach each other.

448. This fact was rendered very obvious by an ingenious and simple variation in the mode of making the experiment. The wires of the battery were made to pass through glass tubes, and the tubes were then placed in the two glasses, which, as before, were connected by the metallic arc. Instead of water, however, both the tubes and glasses were filled with an infusion of red cabbage, which held a neutral salt in solution. As soon as the electricity was put in motion, the neutral salt, in each tube and glass, was decomposed; and the effects were at once conspicuous on the vegetable infusion. For, on the side connected with the positive end of the battery, the fluid in the tube was reddened; while, in the glass of the same side, it was rendered green. On the contrary, the fluid in the tube connected with the negative side was green, and in the glass of the same side it was red. Hence, decomposition had taken place on each side; and while the positive pole of the battery attracted, as usual, the acid, which reddened the infusion in the tube of that side, the negative extremity of the arc attracted the alkali in the glass below, and changed its fluid to a green; and, by the opposite electricities of the respective wires, reverse effects were produced in the fluids of the tube and

glass connected with the negative side of the battery. "These facts, therefore," to repeat the language of Mr Davy, "seem fully to invalidate the conjectures of M. Ritter and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr Cavendish."

449. Such, then, is the general mode in which Galvanic electricity exerts its chemical action ; but let us farther observe, separately and more minutely, what happens at each pole of the battery. First, then, at the positive pole, a particle of water, for example, is decomposed ; its oxygen is attracted, and its hydrogen is repelled. But this is not all that happens ; for the attracted oxygen combines, in most instances, with the metallic wire, and thus we observe not only the act of decomposition, but that of combination also.

450. At the negative pole, the operations are somewhat different. A particle of water is here, also, decomposed ; but its hydrogen is attracted, and its oxygen is repelled. The attracted hydrogen, however, does not combine, like the oxygen at the opposite pole, but passes off in a gaseous form. Hence the electricity, accumulated at the negative pole, is sufficient only to separate the elements of the compound, but not to combine them anew ; and it may, therefore, be said to accomplish decomposition only, while the other pole effects combination also. So truly is this the case, that silver, says Mr Davy, though one of the least oxidable of the metals, easily unites with oxygen when it is positively electrified, while zinc, one of the most oxidable, is incapable of

combining with it, when it is negatively electrified *. In gold and platina, which have a very weak affinity for oxygen, even the accumulation of positive electricity is insufficient to effect oxidation; and hence, when these metals are employed, the oxygen passes off in a gaseous form, like the hydrogen at the opposite pole.

451. The chemical actions, which the Galvanic fluid thus exerts, are equally accomplished by the operation of common electricity. By the concentrated discharge of this fluid, iron-wire, and other metals are speedily reduced to the state of oxides †. The formation of water, by the combustion of hydrogen, and the production of nitric acid by the electrization of atmospheric air, in the great experiments of Mr Cavendish, afford, also, pure examples of combination. On the other hand, by the varied operation of the same agent, the products, thus formed, may again be decomposed. Thus, M. Beccaria revived many metallic oxides by the agency of electricity, restoring the oxide of zinc to its metallic form, and reconverting cinnabar into real quicksilver ‡. So, in the experiments of the Dutch chemists, and of Dr Pearson, water was resolved into its constituent elements, by the agency of the same power ¶; and nitric acid might, doubtless, be made to undergo a similar change, since Dr Wollaston readily decomposed it by Galvanic electricity §. Hence, then, the

* Phil. Trans. 1807.

† Priestley's Hist. Elec. p. 276.

‡ Ibid. p. 277.

¶ Murray's Chem. vol. ii. p. 172.

§ Phil. Trans. an. 1801, p. 428.

positive electricity of the common machine is able, under different circumstances, to effect both decomposition and combination.

452. But the ingenious experiments of Dr Wollaston farther shew, that the negative electricity of the same machine, like that of the Voltaic battery, effects only decomposition. He coated a portion of silver-wire, $\frac{1}{16}$ of an inch in diameter, with sealing wax, and, by cutting through the middle of the waxed portion, exposed a section of the wire. The two coated extremities of the divided wire were then immersed in a solution of sulphate of copper, placed in an electric circuit between the two conductors of a common machine; and sparks, taken at one-tenth of an inch distance, were passed, by means of the wires, through the solution. After one hundred turns of the machine, the wire which communicated with the negative conductor had a precipitate formed on its surface, which was evidently copper; but the opposite wire had no such coating. Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly redissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire. A similar experiment, made with gold wire, in a solution of corrosive sublimate, had the same effect*. These facts, therefore, seem clearly to prove, that, by the common machine, as well as by the Voltaic battery, decomposition only is effected at the negative pole;

* Phil. Trans. an. 1801, p. 429.

while, at the positive pole, decomposition and combination successively take place, according to the nature of the bodies employed, and the intensity with which the electric matter is made to act.

453. After thus tracing the operation of the different electricities, in promoting the chemical actions of decomposition and combination, let us pass to the consideration of those, which the calorific and chemical rays of the solar beam are respectively found to produce. It is known, that, by concentrating the solar rays upon any body, a most intense heat is produced; that metals are converted into oxides; that the diamond or charcoal is made to combine with oxygen, and form carbonic acid; and, in fact, that almost all the combinations, which can be effected by combustion, may be, in this manner, accomplished. Now the portion of solar light which contributes to these effects, must be the calorific rays; for neither the illuminating, nor the chemical rays produce heat, and they are, consequently, incapable of exciting combustion.

454. But these same calorific rays, not only thus cause the combination of oxygen with bodies, but, under different circumstances, they occasion the separation of this element from them. Thus, if a compound combustible be exposed to the concentrated action of the solar rays, its several elements are first separated, and almost, at the same instant, recombine into a new form, according to the nature of the body employed, and the greater or less intensity with which the caloric is made to act; and hence the various gaseous, fluid, and solid products, which the

combustion of the same body, or of different bodies, affords. The calorific rays of light, therefore, like positive electricity, are able to effect in bodies the chemical actions of decomposition and combination, and these in an order depending on the state in which the bodies are presented to their action.

455. But besides these operations, which the calorific rays effect, we have seen, that the decomposition of metallic oxides (431.), of different acids (439.), and especially of carbonic acid, is produced in that part of the prismatic spectrum which is farthest removed from the heating power of light, and, consequently, by the chemical rays, which possess no heating power. In all these cases, however, we observe decomposition only to take place, and no subsequent combination to follow, in the manner in which that operation is accomplished by the calorific rays; for the oxygen is only released from its combination with the metal, with carbon, or with any other substance with which it was previously combined, but no new product is, at the same time, formed, neither are the usual phenomena of combustion exhibited. Hence, therefore, we may remark a striking difference between the action of the chemical and calorific rays of light; for though both species agree in the property of effecting decomposition, the calorific rays alone give rise to new combinations. Here, then, we trace a close analogy between the operations of solar light and of electricity, not only in the general similarity of effect which they produce in bodies, but also in the particular laws of their action; for negative electricity, like the chemical rays, produces only

decomposition in bodies, while positive electricity, like the calorific rays, occasions both decomposition and combination.

456. Farther, the phenomena, which accompany these chemical changes, attest the great similarity in the operations of these subtile agents; for light and heat, the characteristics of combustion, excited by the calorific rays, are the well-known attendants of ordinary electricity. In the low state, indeed, in which Galvanic electricity sometimes effects decomposition, no heat is perceptible; but when it is employed in a state of greater intensity, Mr Davy found it rapidly to evaporate water, and to inflame and volatilize nitrate of ammonia *. Whenever bodies, brought by artificial means into a high state of opposite electricities, are made to restore the equilibrium, heat and light, says he, are the common consequences; and they are, also, the constant result of intense chemical action. But where large quantities of electricity, of low intensity, act, or the combinations are slowly effected, there is an increase of temperature without luminous appearance †.

457. To the exhibition, however, of these phenomena in combustion, the presence of oxygen is necessary; for the calorific rays do not excite combustion *in vacuo*, nor in any gas deprived of oxygen, even when the most inflammable substance is employed. The very same condition is required for the excitation of electricity. Colonel Haldane observed, that, when the Voltaic pile was placed *in vacuo*, its action immediately ceased; that in nitro-

* Phil. Trans. 1807.

† Ibid.

gen gas it did not even commence ; while, in oxygen gas, or in atmospheric air, it acted with energy, and the oxygen disappeared. These facts were confirmed by Mr Davy, who found, that, in gases devoid of oxygen, no Galvanic electricity could be excited ; but it was more or less abundantly developed, when oxygen gas was present *.

458. But oxygen gas is necessary to combustion, in consequence of the great affinity, which, at a high temperature, it exerts towards combustible bases ; and, during the process, it enters into combination with these bases, and its latent caloric is disengaged. In like manner, Dr Wollaston remarks, that the excitation of Galvanic electricity depends on oxidation ; and that the oxidation of the metal is the primary cause of the electric phenomena, and is not occasioned by the electricity itself. In several experiments, in which silver and zinc were plunged in water, holding mineral acids in solution, the zinc was dissolved, and hydrogen gas liberated by decomposition of the water. It would appear, therefore, says he, that, in the solution of a metal, electricity is evolved during the action of the acid upon it †.

459. This explanation, continues Dr Wollaston, receives additional confirmation from comparative experiments made with common electricity ; for, in the experiment already related (452.), the copper, after being precipitated, “ was shortly redissolved by assistance of the oxidating power of common electricity ‡.”

* Murray's Chem. vol. i. p. 590. 1st edit.

† Phil. Trans. 1801, p. 427.

‡ Ibid. p. 430.

So, likewise, by using an amalgam of silver or of platina, which are not liable to be oxidated, he could obtain no electricity. An amalgam of tin, on the contrary, afforded a good degree of excitation. Zinc acted still better; but the best amalgam was made with both tin and zinc, a mixture which is more easily oxidated than either metal separately. In farther proof of this position, he found, that, when he confined a small machine, with its cushion and conductor, in a vessel of common air, electricity was excited; but when he substituted carbonic gas, the excitation was immediately destroyed; and again returned upon re-admission of atmospheric air. The oxidated metal of the rubber, adds Dr Wollaston, is always negative, and so, likewise, in the Voltaic pile, the oxidated zinc is in the same state. From these facts, he concludes, that electricity, in the common machine, and in the Voltaic pile, originates from the same source; and the power of the latter, he adds, is now known to depend on oxidation*.

460. But if, from the foregoing facts, it appear, that, in ordinary cases, oxygen gas is alike necessary to the development of caloric in combustion and to the excitation of electricity; if, in each instance, this gas disappear, and its ponderable matter enter into a similar combination; and if no caloric or electricity be developed unless these chemical changes take place, are we not constrained to believe, that the same subtile matter, which, during combustion, is exhibited in the form of caloric, appears, during electrization, in the guise

* Philos. Trans. 1810, p. 433.

of the electric fluid? For why should oxygen gas be thus essential to the development of electricity, if it be not *chemically* instrumental in affording electric matter? and in what other way can it, in these experiments, be conceived to afford such matter, except by suffering that reduction of its elasticity, which it has been shewn to undergo? If the air were only mechanically concerned, and acted simply as a *conducting* body, or, if the electricity were excited only by friction, then no reason appears why oxygen gas should be thus essential to the operation, and much less why it should undergo such *chemical* changes; for, that these changes are necessary, is proved by the fact, that even oxygen gas itself is unequal to the production of electricity, if a metallic substance, incapable of oxidation, be employed as an amalgam. Surely the conversion of zinc into an oxide by combustion in oxygen, does not furnish any other or better evidence of the extrication of caloric from that gas, than the formation of a similar oxide in electrization affords of the development of electric matter from the same gas, in the experiment of Dr Wollaston. In both cases, the subtile matter is not simply *conducted* by the air, but is *generated* out of it; and, in both cases, therefore, its production must be the *consequence*, and not the *cause*, of oxidation.

461. But while, in these examples, we attribute the production of electric matter to the chemical action of oxidation, we do not mean to say that this is the only, or even the usual way, in which electricity is developed. The experiments of various philoso-

phers sufficiently shew, that electricity is afforded by the simple contact of different bodies, by friction, and by other means, in which no chemical change takes place, and where, therefore, we have no evidence that any chemical action is exerted. But the excitation of electricity, by these mechanical means, does not disprove its development by chemical agency, any more than the extrication of caloric by friction or percussion disproves its liberation in the ordinary process of combustion. We therefore conceive, that the electric and calorific matter may be developed in bodies, both by chemical and by mechanical means ; and we consider this circumstance to yield no little support to that opinion of their similarity, which we have now been endeavouring to maintain *.

462. Thus, then, we see, from the foregoing statement and comparison of facts, that the calorific and chemical rays of solar light severally decompose and combine various bodies, precisely in the same manner, and with the same phenomena, as the different states or kinds of the electric fluid have been shewn to do ;

* In the excitation of common electricity, indeed, both methods are usually called into action ; for the simple friction of the cylinder and rubber yields, in a mechanical manner, the electricity that is supplied by the communication maintained with the earth ; while the amalgam furnishes, at the same time, a chemical source of electric matter, in consequence of the oxidation which it is made to undergo. And as, in combustion, the chemical combination of oxygen is promoted by the communication of heat, so, in electrization, the oxidation of the amalgam must be facilitated by the electricity that is mechanically excited.

and that, in all cases, the same conditions are required for their operation, and the same laws of action are respectively observed. We do not deem it necessary, for our purpose, to institute a comparison between the *physical* properties of these subtile fluids; for it is only with their *chemical* agencies that we are at present concerned. Neither do we venture, from the examples which have now been selected, to decide on the question of *identity* between the two species of *invisible* light, and the two states or kinds of electric matter. Our present object will have been gained, if we have succeeded in shewing such a similarity of *chemical action* between these subtile agents, as will entitle us to infer, that, where similar effects are, in other instances, produced by them, we may reasonably impute to them a corresponding similarity of action.

463. HAVING thus endeavoured to establish the similarity of chemical operation between light and electricity, let us next proceed to an explanation of those decompositions which are effected by solar light. We have seen, that, by the direct agency of the calorific rays (453.), various bodies are decomposed, and their elements recombine into new forms, under which changes the phenomena of light and heat are exhibited. But the decomposition of many bodies is, likewise, accomplished by the chemical rays of light (454.), without the attendant phenomena of heat or luminous appearance; neither do the

separated elements enter into any new combination. In conformity, therefore, with the analogical inference already deduced (462.), we must suppose these chemical rays, like negative electricity, to exert an attractive force towards the inflammable element of the compound, and a repulsive force towards its oxygenous ingredient, whereby, as in electro-chemical operations, these elements are separated, and the decomposition of the body is effected. Whether, in ordinary cases, the calorific rays assist in effecting these decompositions, and to what extent they act, we have not the means of deciding; but as the chemical rays, after their separation from the calorific, appear to be equally capable (442.) of exerting this decomposing power, we must conclude that it is chiefly accomplished by those rays alone.

464. Applying, then, these views to the subject of carbonic acid, we have seen that this gas is formed, in combustion, by the direct agency (453.) of the calorific rays; and so powerful is the union between its elements, that, until the late discovery of potassium, no single substance, at the ordinary temperature of the atmosphere, was capable of effecting their separation. At a high temperature, however, Mr Cruickshank found that a partial decomposition of this acid was accomplished by heating together carbonate of lime and iron-filings, whereby the acid was reduced to the state of an oxide *. In the experiments of Mr Tennant, in which carbonate of lime and phosphorus were submitted to a high degree of heat,

* Nicholson's Journal, 4to, vol. v, p. 4.

the complete decomposition of carbonic acid was effected, and the charcoal appeared in the form of a black powder *. Mr Davy, also, remarked, that potassium readily inflamed and oxidated in carbonic gas †; and Dr Henry and Mr Dalton have likewise ascertained, that, by a continued repetition of electric shocks, this gas may be decomposed, even while it retains its elastic form ‡. In all all these examples, however, the decomposition is effected under circumstances absolutely destructive to the vegetable body; and, except when potassium is employed, caloric, or the intense agency of positive electricity, is called into action.

465. But at the ordinary atmospheric temperature, and even in a degree below *zero* (299.), carbonic acid is decomposed in plants by the power of solar light; and since this decomposition always precedes (378.) the formation of the green colour, and this green colour is effected (437.) by the sole agency of the chemical rays, it follows, that these rays alone are essentially concerned in the operation. If, therefore, these rays, in their chemical action, be considered to resemble negative electricity, they must be held to exert an attraction towards the carbon or inflammable base, and a repulsion towards its oxygen, in the same manner as in the decomposition of water by the negative pole of the Voltaic battery, its hydrogen is attracted and its oxygen is repelled. Consequently, the oxygen of the acid will pass off in

* Phil. Trans. 1791, p. 183. — † Ibid. 1809, p. 73.

‡ Dalton's Chem. Phil. part 2. p. 382.

an elastic form, and the carbon will remain behind in union with the colourable matter of the plant.

466. But supposing solar light thus to act in decomposing carbonic acid in plants, some other agent must be called in to aid its operation; for the experiments of Mr Davy (303.) prove, that light alone is unable to decompose this acid gas; and it is equally certain that the plant itself is unequal to this effect without the agency of light. In the experiments of Mr Tennant and Dr Pearson, in which carbonic acid was decomposed at a high temperature by phosphorus, the acid was employed in a fixed state, while in union with lime or alkali; and it was only under such circumstances that its decomposition could be effected. The elasticity of the acid, in its uncombined state, may be considered as counteracting the affinity of the phosphorus to its oxygen; but in its concentrated state, says Mr Murray, it is more liable to be acted upon, and its decomposition may be farther promoted, by the affinity which the lime or alkali exerts to the phosphorus and oxygen, and in consequence of which they enter into combination*.

467. Now the same bodies which, in these experiments, were employed to reduce the elasticity of carbonic gas, exist abundantly in the juices of plants; and with these alkaline substances, this acid gas will naturally combine. That it does enter into such combinations, may be inferred from the fact, that, though it exist in large quantities in many leaves, it is not abstracted by the air pump, nor expelled by

* Syst. Chem. vol. ii. p. 355.

heat (348.), but is readily given out under the chemical action of solar light; and, indeed, M. Vauquelin and others have detected it in combination with alkaline matter. In this state, therefore, of combination and concentration, it may yield to the decomposing power of the solar rays, although it resisted their intensest action, while retaining its elastic form. And thus we learn, in what manner the chemical condition of the leaf may conspire with the agency of light in promoting the decomposition in question, and why neither the leaf alone, nor the power of light alone, is of itself able to effect it.

468. But after the oxygen of the acid is expelled, the carbon is necessarily retained in the leaves, and serves, probably, some useful purpose in those organs. It has been already remarked (25.), that plants not only owe their green colour, but their odour and combustibility, to the immediate agency of light; and as the chemical rays alone are necessarily concerned in the formation of this green colour, we must, also, suppose that they essentially contribute to the production of those other properties, which are so immediately associated with it. The resinous matter, which imparts this colour, is also the most inflammable part of plants, and that, too, in which, probably, the odour and combustibility principally reside; but in what manner the chemical portion of light immediately acts in the production of this resin, no facts enable us, at present, to state. The celebrated Scheele supposed light to be decomposed in vegetables, and to contribute to the formation of their combustible matter; for the generation of a green

resin in plants, after having been brought out of a cellar, where they are almost white, and standing only a couple of days exposed to the sun, makes this, says he, probable to me * ; and even Newton conjectured, that gross bodies might receive much of their activity from the particles of light that entered into their composition †. Lastly, M. M. Chaptal and Hassenfratz found, that plants, which vegetated in darkness, contained much less carbon than those which had grown in the light ; and M. Senebier also ascertained, that such plants yielded less oil and resinous matter ‡ ;—facts which seem directly to prove the influence of light in increasing the proportion of combustible matter.

469. But whence, it may be farther asked, is the alkali, which so much abounds in the ashes of the green parts of vegetables (377.), and so directly contributes to their colour, derived ? It doubtless pre-exists in the vegetable body, and is not a product of combustion ; for it is found in combination with different acids, and even in an uncombined state, before the process of combustion is instituted ; and that process, as M. Vauquelin has remarked, serves only to develope the saline matter, as the other constituents are consumed. How, then, does this matter gain admission into the living plant ? It is probably held in solution in the fluids, which plants absorb from the

* On Air and Fire, p. 158.

† Optics, Query 30.

‡ Thomson's Chemistry, vol. v. p. 360. 4th edit.

situations in which they grow. Thus, it is known, that terrestrial plants afford potassa, while those which grow in, and near the sea, chiefly furnish soda; but M. M. du Hamel and Cadet found, that, if marine plants be removed to inland situations, they gradually cease to yield soda, and at length potassa only is obtained *;—facts which prove the influence of situation, in determining the kind and quantity of alkaline matter.

470. From the recent investigations of Mr Davy, we learn, also, that almost all solids and fluids, even the purest distilled water, contain saline matter; so that nearly every substance which enters vegetables must convey to them a portion of it. It cannot, however, be supposed, that the alkali is formed in the vegetable directly from its proper elements; for the bases of the alkalis so rapidly abstract oxygen from almost every other substance, that they cannot, as Mr Davy remarks, exist near the surface of the earth in a pure form. And farther, as the alkalis themselves attract acids, with scarcely less avidity than their bases attract oxygen, the same reasoning obliges us to suppose, that, as pure alkalis, these substances do not enter plants; wherefore we must conclude, that they chiefly gain admission, in combination with different acids, that is, in a neutro-saline state †.

471. After their entrance into vegetables, these saline bodies may be presumed to undergo various

* Murray's Chemistry, vol. ii. p. 94, 95.

† Phil, Trans. 1807.

changes ; for, both in terrestrial and marine plants, alkaline matter is found in combination with different vegetable and mineral acids. Hence, therefore, we may reasonably suppose, that these saline compounds are decomposed, and formed anew, at different periods ; and the great quantity of alkali that exists in the leaves, its presence in an uncombined state, and the chemical agency of light to which it is there exposed, all concur to shew, that these changes are principally effected in those organs. By such decompositions, alkaline matter may be developed, even although no oxygen gas be afforded, since the acid that is separated may not be decomposed ; and thus light, by its chemical agency, may contribute to produce the green colour in plants, not merely by decomposing carbonic acid, in the manner already described, but by acting on such other saline compounds as may be present in the vegetable fluids.

472. These views, respecting the entrance of saline matter into plants, and its subsequent decomposition, derive support from some well-established facts, relating to the use of lime in the practice of agriculture. The good effects of this substance, when laid upon soils, endure only for a limited period ; and hence its application is usually repeated every ten or twelve years. In some soils, the lime may, perhaps, act mechanically in improving the texture ; in others, it may act chemically, both in improving the texture, and in facilitating the decomposition of organic matters ; but it may also, we believe, act *physiologically* on the plant itself, by mixing with its fluids, and serving those useful purposes in the vegetable econo-

my which alkaline matter seems destined to perform. The experiments of Mr Davy prove that lime, as well as alkali, is present in the vegetable fluids * ; and it has been shewn to exert similar effects on the colourable juices (401.) of the leaves ; so that we possess evidence, not only of its existence in the plant, but of its capacity to effect a necessary change in the vegetable juices. Such views explain why lime is useful in every kind of soil that is deficient in calca-reous matter ; and account, also, for its gradual disappearance or exhaustion. Is the large quantity of alkali, which green vegetables afford, obtained only in some compound form, from potassa existing in the earth ? or, in the process of vegetation, is lime, which resembles it in so many of its properties, actually *transmuted* into it ? The more hidden secrets of nature are but just beginning to be disclosed ; and the dreams of alchemy are, perhaps, destined to take form and substance, from the researches of modern chemistry.

473. The analogy between the chemical operation of the different electricities of the Voltaic pile, and the calorific and chemical rays of light, which we have thus endeavoured to support, will, we conceive, derive confirmation from the results of certain electrical experiments, which have been made on vegetable matters. For the different electricities rapidly change vegetable colours, by developing, in all cases, as we believe, acid or alkaline matter, precise-

* Phil. Trans. 1807.

ly in the same manner as these changes appear to be more slowly effected by the chemical agency of light.

474. When the electric spark, says M. Cavallo, is taken in the tincture of certain flowers, it produces effects like an acid, which led some of the earlier electricians to consider the electric fluid as an acid * ;—an opinion which has been more lately maintained by M. Brugnatelli, who goes so far as to assert that it actually forms saline compounds by combining with metals †. But this change of colour does not seem to occur when the air is excluded ; for Dr Priestley did not observe it to take place in syrup of violets, when he electrified it in closed glass tubes ‡. Mr Nicholson and Mr Carlisle found an infusion of litmus to be reddened by the positive wire of the Voltaic pile, while it remained blue at the negative one ; and Mr Cruickshank, who observed the same phenomena, supposed the acid, which caused the redness, to be the nitric, and the alkali, which gave a deeper tint of blue at the negative wire, to be ammonia §. Dr Wollaston impregnated a card with a strong infusion of litmus, and then passed along it, by means of two fine gold points, touching the card at the distance of an inch from each other, a current of electrical sparks. In this state, a few turns of the machine were sufficient to produce on the card a redness at the positive wire, very manifest to the

* Ess. on Med. Elect. p. 19.

† Wilkinson on Galvanism, vol. ii. p. 137.

‡ Hist. of Elect. p. 703.

§ Wilkinson on Galvanism, vol. ii. p. 49. 54.

naked eye ; and the negative wire, being afterwards placed on the same spot, soon restored to the card its original blue colour. Similar results were obtained, in less time, by the different electricities of the Voltaic battery *.

475. Now, in all the foregoing examples, there is just reason to conclude, that the changes of colour were, in every instance, produced by the formation, or development of acid or alkaline matter : and the sources from which both might be derived are clearly pointed out in the late refined analyses of Mr Davy. He found that water, after repeated distillation, still contained saline matter, which was decomposable by electrization ; that this matter was likewise obtained from the substance of glass, and from every earthy body ; and, lastly, that the nitrogen, naturally existing in water, by combining with nascent oxygen or hydrogen derived from its decomposition, formed, respectively, nitric acid or ammonia, which were easily discoverable by the proper tests. But when the water which he used was rendered perfectly pure ; when no saline matter existed in the substance of the vessels employed ; and when nitrogen was wholly excluded, by conducting the operation in vessels of pure hydrogen gas, then neither acid nor alkaline matter was either generated or developed, but the water was resolved into its constituent gases alone †.

466. From these facts, we learn why, in Dr Priestley's experiment (474.), air seemed necessary

* Phil. Trans. 1801, p. 432.

† Ibid. 1807.

to the change of colour in the vegetable infusion, since its nitrogen contributed to the formation of nitric acid. In the experiments of Brugnatelli, this acid, also, was probably produced; and either the formation of acid and alkali, or their development by the decomposition of saline matter, is clearly indicated in the experiments of Nicholson and of Cruickshank. In Dr Wollaston's experiment, the acid and alkali were probably derived from saline matter, existing either in the infusion with which the card was impregnated, or in the card itself; for Mr Davy remarks, that the minute quantity of saline compound, contained in paper tinged with turmeric, and which has been plunged in pure water, affords alkaline matter at the negative point of the circuit; and acid, in the same manner, is developed from litmus paper, at the positive point*. All these facts, therefore, afford striking illustrations of the great facility with which acid and alkaline matter may be generated, or developed, by the agency of electricity; and of the capacity of these substances, when thus produced, to act on the colourable juices of vegetables.

477. Nor is it only on these juices, after their extraction from the plant, that electricity is found to act; for results, in some degree similar, follow the operation of the same agent, when applied to the living leaf. Dr Priestley found, that electric explosions, passed over a green leaf, injured its texture, and left a yellow track behind†;—which effect was

* Phil. Trans. 1807.

† Hist. Elect. p. 684.

probably occasioned by the development of acid matter from the saline compounds of the leaf. In an experiment of Mr Davy, where a plant of mint was submitted to electrization in the Voltaic circuit, its saline compounds were actually decomposed; for green colouring matter, with resin, alkali and lime appeared in the negative vessel, and vegetable prussic acid in the positive vessel. In another experiment, a healthy plant of mint, which survived electrization, afforded, in ten minutes, alkali and lime to the negative, and acid matter to the positive side;---facts, says Mr Davy, which shew that the electrical powers of decomposition act even on living vegetable matter*.

478. Since, then, from these facts, it appears, that electricity so readily decomposes the saline compounds in plants; since the separated portions of these compounds so easily act on the colourable juices of vegetables; and since, when light promotes the colouration of plants, it seems to act only through the medium of similar decompositions,---may we not reasonably conclude, that, as these subtile agents have, on other occasions, been shewn to produce so many effects in common, they act also, on these occasions, in a similar manner, and by the exertion of similar powers? The argument from analogy, which we thus desire to urge, does not hinge on one or two points of coincidence, but is borne out by the whole series of successive operations that has been now disclosed; and, if solar light and electricity be allowed

* Phil. Trans. 1807.

to have any one property in common, or to produce any one effect in common, we must think ourselves entitled to conclude, that they exert but one common action in their chemical operation on the saline compounds of plants.

479. BUT granting that Light acts *chemically* on the juices of plants, in the manner that has now been stated, and changes their composition, so as variously to affect their colour, yet, as this action is confined to the *invisible* rays, the actual production of colour cannot be ascribed wholly to chemical agency; and the question, therefore, still returns, How do plants, or the juices of plants, act on the *visible* or *colorific* rays of light, so as to present that rich variety of colours, which they actually exhibit to our view? To give some insight into this matter, it is necessary, as Sir Isaac Newton observes, to understand not only the nature of bodies, but the nature of light; for both, he adds, must be understood, before the reasons of their actions upon one another can be known*.

480. Having, therefore, as he conceived, established the fact, that “bodies reflect and refract light nearly in proportion to their densities,” he proceeded to consider those properties of light which occasion it to be thus affected. These properties he supposed to consist in certain dispositions of the

* Optics, B. ii. part 3, prop. 10.

rays, which caused them, in their passage through any refracting substance, to be alternately reflected or transmitted, at equal intervals, for many vicissitudes; and the returns of these dispositions in the rays, he named fits of easy reflection or transmission. Of what kind, however, this action or disposition in the rays may be, he does not inquire; but suggests the possibility of its depending on a vibratory motion, excited in bodies by the rays which impinge on them. But whether this hypothesis be true or false he does not consider, contenting himself with the bare discovery, that the rays of light are, by some cause or other, disposed to be reflected or refracted for many vicissitudes*.

481. As thus, the mechanical hypothesis, even in the hands of Newton, seems to fail in assigning the cause, why light is variously refracted and reflected by bodies, we may, perhaps, be permitted to speculate on the facts and analogies, which the late discoveries in the elementary constitution of bodies, and in the nature of light, suggest to us; and try whether they will conduct us a step farther in the investigation of this intricate subject. “*Prudens interrogatio,*” says Lord Bacon, “*est dimidium scientiæ. Vaga enim experientia, et se tantum sequens, mera palpatio est, et homines potius stupefacit quam informat.*”

482. From the view which has been given (440. et seq.) of the nature and composition of solar light, it appears, that although, in many of their physical

* Optics, B. i. part. 3. prop. 12.

properties, the three species or portions of light agree, yet, in other properties, they very sensibly differ. We have already considered the nature and operation of the *invisible* portions of the solar beam, which effect chemical changes in bodies; and we have now to direct our attention to that portion which is *visible*, and imparts the sensations of light and colour. In the investigation of complicated actions, it is only by thus analysing and breaking them down into separate and distinct parts, that we can hope to discover the full reason and effect of their combined operations.

483. Many examples have been already given of the reciprocal action which bodies and the invisible portions of light exert on each other; and the phenomena of phosphorescence indicate the exertion of a similar action between bodies and the luminous or visible rays. So, likewise, whether the light afforded in combustion come from the air, or from the combustible body, it equally manifests in bodies the existence of a portion of the luminous rays. These rays, however, although thus retained in bodies, appear to exert on them no chemical action; for the body exhibits no apparent change of properties, whatsoever be the colour of the light which it retains, or emits. The emission of light, also, by vegetable and animal substances under putrefaction, is not the cause, but the consequence, of that process; neither does it appear, that the light afforded by certain living animals is necessarily productive of any chemical change in the animal, or in the air that surrounds it; but merely accompanies those changes, which are necessary to the exercise of the vital func-

tions. Farther, we have seen, that the chemical changes of decomposition and combination are effected by invisible light, in which changes the luminous or visible portion has no necessary share.

484. But in the operations of invisible light on bodies, we have traced a close analogy to the actions of the electric fluid; and may we not extend this analogy to those affections of the luminous or colorific rays, which enable bodies to present all the endless modifications of colour? Such an idea, however, at once presupposes the exertion of an attractive force between bodies and this portion of light; and should the circumstances already stated be deemed insufficient to establish this fact, the phenomena afforded by inflection seem decisive of the question. It is well known, that if different bodies be made to approach a beam of light, as it enters a darkened room through a small hole in the window, the light will be drawn out of its rectilinear course. Sir Isaac Newton found that this effect was produced not only on the entire beam, but, in different degrees, on the different colorific rays; for the fringed shadows of bodies held in red light were larger than those of bodies held in green light; and these last were still larger than those of bodies held in the violet rays: so that the same body, says Newton, acted upon the red, or least refrangible ray, at a greater distance than on the violet, and by those actions disposed the red light into larger fringes, and the violet into smaller, and the lights of intermediate colours into fringes of intermediate sizes*. Surely

* Optics, B. iii.

no experiments can more clearly shew, not only the general fact of an attraction exerted by bodies upon the entire beam of light, but also upon the individual colorific rays, of which that beam is composed ; for when one species of matter is thus drawn out of its course by another, in what language can we express the fact, if we do not call it attraction ? How, then, must we suppose this attraction to be exerted ?

485. “ Have not the small particles of bodies certain powers, virtues or forces,” says Newton, “ by which they act at a distance, not only upon the rays of light for reflecting, refracting, and inflecting them, but also upon one another, for producing a great part of the phenomena of nature ? For it is well known that bodies act one upon another by the attractions of gravity, magnetism and electricity.” “ How these attractions may be performed,” he continues, “ I do not here consider. What I call attraction, may be performed by impulse, or by some other means unknown to me. I use that word here to signify only, in general, any force by which bodies tend towards one another, whatsoever be the cause. For we must learn, from the phenomena of nature, what bodies attract one another, and what are the laws and properties of the attraction, before we inquire the cause by which the attraction is performed. The attractions of gravity, magnetism, and electricity, reach to very sensible distances, and so have been observed by vulgar eyes ; and there may be others which reach to so small distances, as hitherto escape observation ; and perhaps,” he adds, “ *electrical attraction* may reach to such small distances, even without

being excited by friction *.” Thus, then, we see, that, although in his inquiries concerning the motions of light, Newton makes no mention of attraction, yet, in his profound meditations on the general phenomena of nature, he not only employs that term, but points to the very species of attractive force, which the foregoing investigations have suggested ; and, with almost prophetic sagacity, has foretold the existence of those “ virtues or forces” in the elementary particles of bodies, on which the researches of Mr Davy have rendered it probable that their attractive energies depend.

486. From these researches, it would appear, that the elements of matter naturally possess different electrical states, which respectively determine their attraction or repulsion by the opposite poles of the Voltaic battery ; and these natural electricities Mr Davy supposes them to retain in all the combinations into which they enter †. If this doctrine be just, and if the rays of light do obey laws similar to those of the electric fluid, there is no difficulty in conceiving that the particles of matter may exert different forces upon the luminous rays ; and hence their refraction would vary both according to the natural electricities of the elements of the body, and to the properties of the individual rays ; and the modified operation of such forces might be conceived sufficient to account for the varied refraction and extinction of the different rays of light.

* Optics, Query 31.

† Phil. Trans. 1807.

487. But if it were granted that the refraction and extinction of certain rays depended on some power which resembled electro-chemical attraction, yet the admission of such a power would not explain the phenomena of reflexion. Hence, should we admit, says M. Hâüy, the existence of an affinity between light and the particles of matter, we should still be embarrassed in reconciling the repulsive force, which reflexion indicates, with affinity, which is an attractive force *.

488. If, however, we call to mind the motions that arise in bodies submitted to electrization, in which we observe attractions and repulsions not only to succeed each other with great rapidity, but frequently to exist together, we can have no difficulty in conceiving that certain rays may be refracted and extinguished in bodies, at the same time that others are reflected. Sir Isaac Newton himself, indeed, considered reflexion to be caused, not by the solid parts of bodies, but by some subtile matter which intercedes their pores †; and surely all the phenomena of electricity and of magnetism lead us to believe, that, where repulsion is ascribed to such a power, the corresponding attraction must be attributed to the exertion of a similar force. “Do not bodies and light,” says Newton, “act mutually upon one another, that is to say, bodies upon light, in emitting, reflecting, refracting, and inflecting it, and light upon bodies in heating them?” &c. “And do not the rays

* *Traité de Phys.* tom. ii. p. 264. 2d edit.

† *Optics*, B. ii. part 3. prop. 8.

of light, which fall upon bodies, and are reflected or refracted, begin to bend before they arrive at the bodies, and are they not reflected, refracted and inflected by one and the same principle, acting variously in various circumstances * ?” If the view which has now been suggested be admitted, this question must be answered in the affirmative ; for the various phenomena of refraction, reflexion, and inflexion, will fall to be regarded as modifications of electrical action. And thus have we endeavoured to connect the truths, which the intellectual eye of Newton perceived at so vast a distance, with the facts which the discoveries of Davy seem to have brought so immediately within our view.

489. But were the physical cause of refraction and reflexion thus attributed to electro-chemical agency, still the laws which these motions of light observe, become the subjects of mathematical consideration. Granting, also, that these motions arise from the varied exertion of attractive and repulsive forces, yet still the greater mass of matter, or its greater density, would necessarily augment the effect, even where the composition of the substance continued uniform ; and a change in composition would act still more powerfully, and independently, in some degree, of the quantity of matter. In consistency, therefore, with these views, the Newtonian doctrine of density, in varying refraction and reflexion, may still, to a certain extent, be considered just ; but the mass of matter, or its density, would, in such an hypothesis,

* Optics, Queries 4 and 5.

be regarded rather as conditions which modify, than as causes which produce the phenomena : and hence the mechanical properties of bodies would, in such actions, be considered subordinate to those which arise from their chemical constitution.

490. But whatever hypothesis may be adopted concerning the cause of refraction and reflexion, the certainty that these effects are produced, and that to them the colours of natural bodies are attributable, will not be disputed. It only remains, therefore, that we apply our knowledge of these subjects to the explanation of those colours with which nature has adorned and embellished the vegetable world.

491. The green colour which plants exhibit may be either simple or compound. Newton remarks, that a leek appears most resplendent when viewed by green light, and next when seen by the blue and yellow rays which compound a green *. In the opinion of M. Berthollet, the green colour of plants is produced by simple and homogeneous light, and not by a compound of the yellow and blue rays † ; but M. Haüy asserts, that, if a slip of a green plant be laid on yellow paper, and held between the light and the eye, and the paper be then agitated so as to aid the sensation, the green slip will appear more or less blue, which proves, says he, that the green colour is a compound of yellow and blue, and not a simple colour ‡.

* Optics, B. i. part 2. prop. 10.

† Elem. de la Teinture, tom. i. p. 12.

‡ Traité de Phys. t. ii. p. 64. 2d edit.

492. This experiment resembles those made by M. Buffon, who found, that, by looking for some time on two objects of opposite colours, fixed upon one another, the sensation of a third colour, different from either of the former, was produced, to which he gave the name of *accidental colour*. The production of this colour has, by some philosophers, been attributed to a mixture of the differently coloured rays reflected by the two objects, combined with a certain disposition in the eye, to be acted on by the colour which conveys the stronger impression. But the experiments of M. M. *Æpinus* and *De la Hire*, seem to shew, that sensations of different colours may successively arise in the eye, where no mixture of colours from the object can be supposed to take place: thus, if the eye be directed to the sun, and then closed, the image upon the retina will be first red, then yellow, then green, and last of all blue*; —facts which shew that these accidental colours depend more on the condition of the organ of vision, than on the colours of the rays which the luminous body emits. The same explanation may, perhaps, be extended to the *accidental* colours produced by reflected light; for by looking long and steadily at two differently coloured objects, kept in perpetual motion, the eye becomes dazzled and weakened, and is unequal, therefore, to that distinctness of vision, which at first represented them in their proper colours. According to the natural strength, also, of the visual organ, the same objects may, to different

* Priestley's Hist. Vision, vol. ii. p. 631.

persons, present different colours ; so that no proof of the compound nature of the green colour of plants can be drawn from this experiment, relating to *accidental* colours, as M. Haüy seemed to suppose.

493. The operation of electricity on the organ of vision, supports the view above stated, respecting accidental colours ; for when the eye is positively electrified, objects are said to appear red, large and distinct ; and when it is in a negative state, they look blue, small and obscure *. M. Ritter placed his eye in contact with the negative pole of the Voltaic pile, when it acquired the opposite state of electricity, and the objects all appeared of a red tint ; and afterwards, on applying his eye to the positive pole, which caused the retina to pass to a negative state, the objects all assumed a purple hue. Thus, the organ of sight, says M. Haüy, subjected to the combined action of light and electricity, becomes, as it were, a common link joining together phenomena observed till then in isolated experiments, and with instruments which appeared to have no affinity with each other †. And do not these similar sensations of colour, produced by the sun's light and by electricity, support the analogies traced, in so many other instances, between the operations of these subtile agents ?

494. The mode, also, in which the green colour is formed in vegetable infusions, leads to the belief that it is truly a simple colour. When an alkali is added gradually to a colourless infusion, the liquor,

* Haüy's Elem. Phil. vol. ii. p. 55.

† Elem. Phil. vol. ii, p. 260.

at first, is violet, then blue, and then green, the colours emerging in the order of the refrangibility of the rays; and as, in this order, the green stands before the yellow, it is reasonable to suppose it to be, in this case, a simple colour, caused by the transmission of the green rays, rather than a compound of the less refrangible yellow with the blue. Add to this, that many of the vegetable colours, as the blue, the red and the yellow, must necessarily be simple, because they cannot be formed out of any other colours. But though the green be thus, for the most part, considered simple, yet it may, in some instances, be compounded, as must be the case with the colours of a vast variety of flowers, which present such an endless variety of hues.

495. As a general summary of the conclusions, which, in the foregoing discussions, we have endeavoured to establish, respecting the agency of light in promoting the colouration of plants, we may observe, in the first place, that, by the chemical agency (465.) of this subtile matter, the saline compounds of plants are decomposed, and the acid and alkaline matter, thus developed, combine with the colourable juices of the vegetable. In consequence of this combination, these juices are enabled to act variously on the luminous rays. When the alkali predominates, the more refrangible rays, as the violet, blue, and green, are reflected, and the other rays are extinguished; when the acid prevails, the least refrangible, or red rays, are reflected, and the others disappear; and from intermediate admixtures of these ingredients, intermediate colours, both simple and compounded, will a-

rise. The colours, however, which these juices present to our sight, are not reflected by the coloured particles (417.), but by the opaque matter on which they are imposed, so that the coloured matter transmits only, but does not reflect light; and this light arriving at the eye, produces an impression, which conveys the sensation of the individual colour.

496. Hence, too, it follows, that, when light is wholly excluded, the chemical changes in the vegetable juices, which enable them to exert these actions on the colorific rays, do not take place, and, consequently, the green colour of the leaves, which depends on the predominance of alkaline matter, and the red colours of leaves and of flowers, which arise from an excess of acid, are equally prevented from appearing; for the juices being unable, in this state, to decompose the solar beam, return it almost or entirely unchanged to the eye, whence the objects are destitute of colour, or have the appearance of whiteness. The colours of plants, therefore, depend primarily on the chemical action of light, in changing the constitution of their juices; and these juices, by their physical operation on the colorific rays, are then enabled to exhibit all their infinite variety of hues.

497. We are fearful that some of our readers may think we have extended these remarks on the agency of light too far, and that many of the observations, which have now been made, are but remotely connected with the professed objects of our inquiries. We entreat them, however, to suspend their judgment,

till they shall have accompanied us farther in our progress, and are thereby better enabled to appreciate the importance of the discussions in which we have been engaged. If Physiology, and the arts of Agriculture and of Medicine, which so much depend upon it, be ever destined to pass the narrow bounds which are at present drawn around them, to rise from facts to principles, and from effects to their causes, it can only be by the large and liberal cultivation of almost every branch of natural knowledge. “Interim nemo expectet magnum progressum in scientiis, (præsertim in parte earum operativa), nisi philosophia naturalis ad scientias particulares producta fuerit, et scientiæ particulares rursus ad naturalem philosophiam reductæ. Hinc enim fit, ut plurimæ artes mechanicæ, atque ipsa medicina, atque (quod quis magis miretur), philosophia moralis et civilis, et scientiæ logicæ, nil fere habeant altitudinis in profundo ; sed per superficiem et varietatem rerum tantum labantur.”—“Itaque minime mirum est, si scientiæ non crescant, cum a radicibus suis sint separatae*.” It is under the impression of these truths, that, through the whole of our inquiries, we have freely sought assistance from every department of science, to which we were able to apply,—convinced, that “that philosophy beareth best its own name, which doth not strain all to two or three principles, like two or three bells in a steeple, making a pitiful chime, but trieth to rise up to Nature’s

* Nov. Organ. lib. i.

own number, and so to ring all the changes in the world *."

SECT. V.—*Of the supposed Utility of Carbonic Acid, and of the Decomposition of Water in Vegetation.*

498. FROM the circumstances of carbonic acid being decomposed in plants, by the agency of the solar rays, many writers have been led to consider that gas as necessary to vegetation in sunshine; and others have extended this opinion so far, as to suppose it essential to the growth of all plants through the day. In our former work, we combated the evidence (43.) on which this opinion was founded; but as new experiments have been brought in its support by M. de Saussure, and as, from the view which we have now taken of the phenomena of vegetation in sunshine, our former notions may be expected to have undergone some modification, we shall re-examine, as concisely as possible, the more material evidence relating to this question, with the hope of removing some of the difficulties, and apparent contradictions, which, at present, seem to attend it.

499. Is then carbonic acid essential to the vegetation of plants? We have no direct experiment, says M. de Saussure, by which this is demonstrated, and many facts seem even to establish the contrary.

* Grew's *Anatomy of Plants*, p. 223. an. 1682.

It is known, for example, that if plants be put to vegetate in a vessel filled with atmospheric air, deprived of its carbonic acid, and the vessel be then exposed to the sun, the plants continue to flourish, and the air experiences no change, either in purity or in volume *. These experiments he repeated many times, and always found the plants to grow as well in air, from which the carbonic acid had been previously removed by lime-water, as in those cases where this operation had not been performed †; from whence it necessarily follows, that the presence of carbonic acid in atmospheric air is not essential to the vegetation of plants.

500. But, from experiments of a different nature, M. de Saussure was led to draw an opposite conclusion. In vessels in which plants were placed to vegetate, he suspended a quantity of lime, which had been slaked by water, and afterwards dried briskly at a boiling heat; the vessels were, also, inverted into saucers of lime-water. After the second day, the atmosphere of the plants, exposed to the sun in this apparatus, diminished in volume. On the third day, the inferior leaves began to turn yellow: by the fifth day, they had all fallen, and the plants no longer exhibited signs of vegetation. Their atmosphere, at this period, had lost in bulk three cubic inches of the fifty originally employed; it was less pure than at first, but contained no carbonic acid. Sometimes the plants died in two days, when thus exposed to the sun; while others, confined in similar ves-

* Annales de Chimie, t. xxiv. p. 136. † Ibid. p. 144.

sels without lime, continued fresh and vigorous. These experiments with lime prove, says M. de Saussure, an attraction, and, consequently, a formation of carbonic acid by plants which vegetate in sunshine; from whence we collect, that plants growing in the light, form carbonic acid with the oxygen of the atmosphere *.

501. The foregoing conclusion seems to be fairly deduced from the results of the experiments; but M. de Saussure farther considers them to prove, that the presence of carbonic acid is necessary to vegetation, because the plants ceased to vegetate, apparently from this acid being withdrawn †. This inference, however, is completely at variance with the fact, that vegetables grow in air that has been entirely deprived of its carbonic acid (499.); and it is also contradicted by other experiments of a similar nature, conducted in the shade. For in the shade, says M. de Saussure, not only have the plants not died in recipients which contained lime, and were inverted in lime-water, but they have flourished better than in similar recipients which did not contain that substance. In ten days, a plant, confined with lime in the shade, acquired seven grains in weight; the volume of air lost four and a half cubic inches: it was become very impure, and contained $\frac{3}{100}$ of carbonic acid ‡. But in the recipient without lime, the plant acquired only five grains in weight, and the atmosphere contained $\frac{11}{100}$

* Annales de Chimie, tom. xxiv. p. 144. 145.

† Ibid. tom. xxiv. p. 145. ‡ lb. t. xxiv. p. 146.

of acid gas *. Thus, then, we see, that lime, which is considered to destroy plants by abstracting the carbonic acid from their atmosphere in sunshine, is actually favourable to vegetation when that process is conducted in the shade. In what way, then, does lime, in these different experiments, affect the vegetation of plants, and from what cause does this remarkable difference in its action proceed?

502. It appears, that, in the experiments made in sunshine (500.), the carbonic acid was entirely attracted by the lime; but in the shade (501.), the atmosphere still contained $\frac{3}{100}$ of that gas. Did then the plants die in sunshine, because their atmosphere contained no carbonic acid? and did they live in the shade, because a small portion of carbonic acid was present in it? If the first of these positions were true, then vegetation should cease in air deprived of carbonic acid (499.), which is not the fact; and, contrarily, if the second were true, then plants should not grow in air deprived of this acid gas, which nevertheless they are found to do. Consequently, the presence of carbonic acid is not essential to the vegetation of plants, even in sunshine, neither is it injurious to those which grow in the shade; and since, therefore, neither the presence nor the absence of this gas materially affected the vegetative process in these experiments, we must seek out for some other cause of the contrariety in result which they afford.

503. Now this cause must be connected with some circumstances arising out of accidental situation; for

* Recherches Chim. p. 36.

no other difference, in the conduct of the experiments, is stated, than that the plants were respectively placed in sunshine and in the shade. The atmosphere of the plants in sunshine, at the time of their death, between the 5th and 6th day, contained $\frac{1.6}{100}$ of oxygen gas *, which is probably much more than existed in the vessel placed in the shade at the end of the 10th day ; so that the cause of death, in the former case, could not have proceeded from the absolute deterioration of the air. Neither can we ascribe the death of the plants to the abstraction of moisture by the lime, as we had formerly supposed (43.) ; for it is not stated that the leaves had become dry, and their fall might take place from causes entirely independent of their desiccation (233.). Moreover, such a cause would have equally affected the plants which grew in the shade.

504. As, then, neither the state of the atmosphere, nor the condition of the plants as to moisture, seems sufficient to account for their death, may we not suppose that the lime itself exerted some deleterious action in sunshine, which it did not produce in the shade ? This is rendered highly probable by the observations which M. Braconnot has made on these very experiments. He contends, that the death of the plants was not owing to the privation of carbonic acid alone, but to the action of the lime itself in a state of vapour ; for he found, that if litmus-paper was suspended in a phial that contained moistened lime, it, in a short time, was changed to a blue colour,

* Recherches Chim. p. 35.

proving the volatilization of the lime *. We have seen, too, the deleterious effect which mercury, in a similar manner, produces (310.); and this mode of explaining the fact furnishes a reason why the death of the plants happened only in sunshine, since the lime, in that situation, was much more readily volatilized.

505. In the foregoing experiments, the roots of the plants were immersed in pure water, but, in subsequent trials, M. de Saussure operated on the branches of a tree, whose roots were growing in the earth. Into a glass balloon, of the capacity of 200 cubic inches, he introduced an ounce of lime, and, to remove all objection respecting its property of attracting moisture, he lightly moistened it with water. A branch of honeysuckle, or of the peach, furnished with leaves, was then passed into the balloon, and so disposed that its leaves should neither touch the lime, nor the sides of the vessel. The neck of the balloon was then carefully luted to the branch, and the apparatus was exposed to the sun. The leaves of the branch continued green twelve days, when they began to dry, and at the end of three weeks they had all fallen. The branch, however, did not die; but in a month after, while retained in the balloon, which still remained closed, it put forth new leaves. A similar branch, introduced into another balloon, and placed in the same circumstances, but without lime in the vessel, preserved its freshness for more than two months. The

* Nicholson's Journal, vol. xviii. p. 22.

lime, in the former experiment, was found to be saturated with carbonic acid ; and since, in these experiments, the atmospheres still contained a considerable portion of oxygen, it is concluded, that the fall of the leaves was not owing to the want of that gas, but to the absence of carbonic acid alone *.

506. It may be remarked, that, in these last experiments, the effects produced on the plants are very different from those before related, as is also the time required for their completion ; for, in the first experiments (500.), the leaves soon became yellow, and in a few days fell off ; while in the second, they continued green, became dry, and did not fall entirely till the end of the third week. Now, it is very doubtful whether the mere abstraction of carbonic acid would occasion either the one or the other of these states of the leaves ; and it is in no respect probable that it should produce them both. In fact, the results of these two series of experiments correspond with those which we have related (43.) on a former occasion ; and although M. de Saussure states, that, in the latter series, he previously moistened the lime, yet he did it only slightly ; and as evaporation from the leaves was soon checked by their confined situation, they could not have become dry, but from the attraction of their moisture by the lime. While, therefore, the effects on the plants were probably produced, in the first experiments, by the vaporization of the lime, it appears to us almost certain, that, in the last examples, they arose from the abstraction of

* Recherches, p. 38. 9.

moisture, both from the plant itself, and probably from the atmosphere in which it was confined.

507. But farther, according to M. de Saussure, plants in the shade grow better in vessels which contain lime, than in those in which this substance is not present. The carbonic acid, which they then form, is, in great part, attracted; but we are not able, says he, to judge of the effect of a total privation of this acid, because it is formed in too great quantity to be entirely removed; the effect, however, of a partial privation, he adds, is to favour vegetation *. We have, however, given examples (44.) of the total privation of this acid gas from plants vegetating in the shade, without its producing any apparent change in their vegetative powers; and, on the contrary, we have related other experiments, in which the carbonic acid, formed in vegetation, was allowed to remain, without apparently producing any suspension of that process, except in so far as the oxygen gas of the air suffered diminution; so that, in the ordinary vegetation of plants in a given portion of atmospheric air, neither the presence nor the absence of the carbonic acid which they form seems materially to influence the process. This inference is still more certain with regard to plants which grow naturally in the open atmosphere, where the proportion of carbonic acid is always nearly uniform, and seldom or never exceeds 1000 *per cent*.

508. In certain circumstances, however, lime is so far from promoting vegetation, even in the shade, that it altogether arrests that process, as appears from the

* *Recherches Chim.* p. 37.

following experiments. A young bean plant, growing in a garden pot to the height of six inches, was inclosed by the tin apparatus, in the manner already described (226.); and by the side of the plant was placed an egg cup, which contained two drachms of freshly pounded lime. Over the plant and cup, a glass jar, containing about 38 cubic inches of atmospheric air, was then inverted, and the mouth of the jar was surrounded by lime-water; the temperature of the room was 61° , and the apparatus was set aside in a part of the room that received the full light of day, but was not exposed to the direct rays of the sun. During the first day, the jar continued perfectly dry, the plant was rather diminished in height, and the lime had considerably increased in bulk. By the end of the second day, the appearances were nearly the same, but the lime was now much more bulky; and the water had risen into the jar to the height nearly of half an inch. Through the two succeeding days, the lime continued to swell, the water to rise, and the jar remained dry; but the plant, though looking fresh and green, exhibited no sign of growth. At the end of the fifth day, the leaves became pale, and had a withered appearance, and the stem began to turn black. By the close of the following day, the lime was so much swollen as nearly to fill the egg cup; the stem of the plant was more black; the leaves were much paler and more withered, and had fallen two inches below the thread, which was at first tied round the jar to mark their greatest height; and, notwithstanding the water had, by this time, risen nearly an inch, the jar still continued quite dry.

509. In this experiment, therefore, we observe a gradual abstraction of moisture, both from the plant, and from the atmosphere in which it was confined, so as entirely to arrest the growth of the plant, and in a few days to destroy its life. During the whole experiment, the roots were abundantly supplied with water from the dish in which the pot of earth stood; but this was insufficient to counterbalance its attraction by the lime. The gradual rise of the lime-water into the jar indicated the formation and subsequent attraction of carbonic acid; but, nevertheless, the atmosphere had not undergone any change, except from the abstraction of moisture, which could occasion the death of the plant; for it was found, on analysis, to contain more than half its original quantity of oxygen. Hence the death of the plant must be referred to the abstraction of moisture partly from the plant itself, and partly, as we believe, from the atmosphere which surrounded it, by which the air was rendered incapable of supporting vegetation.

510. This conclusion will appear more evident from the result of another experiment made at the same time with a similar plant, and precisely under the same circumstances, except that the lime, instead of being used in the form of dry powder, was slaked, and reduced to the consistence of a thick fluid by the addition of water. In this case, the jar was rendered dim by moisture, and continued in that state through the whole experiment; the plant had grown half an inch in height by the end of the first day; and the water had risen into the jar about three-tenths of an inch. During the two following days, the plant continued

to grow, and the water to rise; but on the fourth day, two black spots appeared on one of the leaves, which gradually spread, and a fine mould began to form. At the end of the sixth day, two of the leaves had become black, and the rest, though green, looked sickly; the slaked lime had now become nearly dry, but the jar was still dimmed by moisture; the plant had grown in all about an inch in height. Hence, by the gradual rise of the water, it appears, that the lime, as in the former case, attracted the carbonic acid which the plant formed; but the dimness of the sides of the jar shewed the continued presence of moisture in the air. The chemical changes which the air had undergone were the same nearly in both cases; and, therefore, the difference in appearance which the plants exhibited can be ascribed only to a different state with regard to the moisture, both of the atmosphere and of the plant. Whether the sickly state of the vegetable, towards the close of the experiment, is to be ascribed to the deleterious action of the lime, or to the stagnation of its fluids, in consequence of its confined situation, we do not undertake to say; but there is no reason to think that the air had experienced any chemical change, sufficient to account for the decay and decomposition which had evidently begun to take place.

511. In order to prove, that, in his experiments, the lime did not affect the plants from any operation peculiar to it, but that of abstracting carbonic acid, M. de Saussure states, that he sometimes employed potassa; but as he has not given the details of any experiment made with this substance, we are unable

to judge of the conclusiveness of the methods which he employed. He only says, that lime or potassa, which destroys the thin leaves of plants which vegetate in sunshine, does not exert this action upon the vegetation of the *cactus opuntia*, and other plants which have thick leaves; because, says he, their thick parenchyme, and the little porosity of their epidermis, enable them to retain obstinately the carbonic acid which they have formed *. We must not, however, conclude, that the life of the cactus is preserved, in consequence of its thus retaining carbonic acid, but in consequence of its hardy constitution, which enables it to resist the deleterious operation of the substances to which it is exposed †.

512. Lastly, we may observe, that, even in sunshine, it is to the presence of oxygen gas (309.), and not to that of carbonic acid, that the vegetation of the plant is immediately to be ascribed; and the presence of this acid, in any considerable quantity, is actually injurious to vegetation (309.), unless it be decomposed by the agency of solar light. But by its decomposition, this acid does no more than afford oxygen; consequently, it is useful in vegetation, not as carbonic acid, but only as it affords oxygen gas. If, therefore, this gas be supplied from any other source, carbonic acid is not required to afford it; and hence vegetation goes on in atmospheric air, though it contain not more than $\frac{1}{1000}$ part of carbonic acid, and even, as M. de Saussure has shewn (499.), when that small portion is removed.

* Recherches Chim. p. 89. † Ibid. p. 64. Note.

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* Thomson's Syst. Che

In as far, however, as carbonic acid is decomposed by solar light, and thereby yields oxygen gas, in so far may it be considered useful to vegetation; but it is not, under any circumstances, necessary to that process, if oxygen gas be duly supplied from any other source.

513. BESIDES furnishing oxygen gas, by the decomposition of carbonic acid, many writers have supposed that the leaves of plants afforded it, also, by the decomposition of water. This opinion, however, seems rather to have been inferred from reasoning, than proved by experiment. Vegetables, it is said, augment their bulk, when furnished only with water and air; and since oxygen and hydrogen (the constituents of water) are likewise two of the most abundant ingredients in plants, it is inferred that water suffers decomposition, and is thereby enabled to contribute to the nutrition of the plant. Because, also, the leaves of plants contain inflammable matter, and, in certain circumstances, emit oxygen gas, M. Berthollet was led to suppose, that, when exposed to the sun, they possessed the power of decomposing water, as well as carbonic acid. In this manner, he believed that the oxygen was derived from two sources; while the hydrogen of the water and the carbon of the acid contributed to form the resinous or inflammable matter*. It is the evidence adduced

* Thomson's Syst. Chem. vol. v. p. 362. 4th edit.

in support of this supposed decomposition of water, and consequent production of oxygen, that we propose now to examine.

514. It has been said, that, when the leaves of plants are exposed to light, in contact with pure water only, they afford oxygen gas,—a proof that their vessels are able, when assisted by the action of light, to decompose a portion of the water which they absorb *. This oxygen, however, may proceed from so many other sources, that the quantity afforded cannot be received as proof that it is derived from the decomposition of water; and the small portion that is yielded by leaves, when thus placed in water, may, with greater probability, be ascribed to the air actually existing in the substance of the leaf before its immersion.

515. But an experiment of Mr Davy seems to afford the best evidence that has been adduced on this subject. He filled a glass cylinder, of the capacity of ten cubic inches, with mercury; and then conveyed into it two small vine leaves, which were passed through the mercury, so as to detach all atmospheric air from them. The mercurial apparatus was now inserted in a vessel of cold water; and aqueous gas, furnished from another vessel containing water, which had been long in ebullition, was passed through a long tube into the cylinder, where it was condensed by the cold mercury. In this manner, the cylinder was filled with water that held no air in solution, and, in this state, it was exposed to light. In a very

* Murray's Syst. Chem. vol. iv. p. 76.

short time, air globules began to form on the leaves, and in about six hours sufficient air was collected to be examined; it measured two cubic inches and a half, and was nearly pure oxygen gas. Since, in this experiment, no gas of any kind was held in solution by the water, and pure oxygen gas was produced, it must have arisen, says Mr Davy, from the decomposition of the water, by the combination of its oxygen with light, and of its hydrogen with the vegetable*.

516. But although it be granted that the water employed, and the surfaces of the leaves, were, in this experiment, freed from air, yet the aëriform fluid in their substance was not expelled. That such a fluid exists in leaves, has been clearly established; and, indeed, Mr Davy's own experiments shew, that, when leaves are confined either in nitrogen or in hydrogen gas, they afford oxygen gas in sunshine. Thus, a small plant of minianet, exposed to the sun in twenty-eight cubic inches of hydrogen gas, yielded two and a half cubic inches of oxygen; and in experiments made with leaves in nitrogen gas, six-tenths of a cubic inch of oxygen were obtained†. Mr Davy, indeed, supposed this oxygen to proceed from the decomposition of the water of the plant; but this is to take for granted the thing that requires to be proved; and the supposition cannot be received, until the source of fallacy just stated has been first disposed of.

517. If water were decomposed by plants, no good

* Beddoes's Contributions, p. 159. † Ibid. p. 154. 163.

reason occurs why its hydrogen should not be liberated, as well as its oxygen is supposed to be; for oxygen enters more abundantly into the composition of the vegetable than hydrogen does, and may, therefore, be supposed to possess an affinity equally strong for the other elements of the vegetable substance. Neither, if we admit the liberation of oxygen by these means, does it follow, that the mere absence of hydrogen is sufficient to authorise the conclusion that it has actually combined with the plant, and been converted into vegetable matter; for, as far as observation yet goes, organised bodies do not appear to be increased in bulk, by matter supplied in an elementary form, but in a state of combination; and the augmentation, which vegetables receive by the function of assimilation, seems to be very different from the mere aggregation of particles, which form an inanimate substance by the operation of chemical affinity, or by the laws of crystallization.

518. The subject of the decomposition of water in vegetation has particularly engaged the attention of M. de Saussure. He observes that plants, vegetating, by the aid of distilled water, in oxygen gas, or in common air deprived of its carbonic acid, augment their weight. This increase of weight, however, affords, says he, no proof of the decomposition of the water, nor of the fixation of its elements, since it may be owing to the mere introduction of that fluid into the vegetable; and, from many experiments, he satisfied himself, that, when plants are reduced to the same state of dryness before and after the experiment, this apparent augmentation of their weight is

little, if at all perceptible. He placed three plants of *lysimachia vulgaris*, weighing $129\frac{1}{4}$ grains, in a recipient containing 250 cubic inches of common air deprived of its carbonic acid, the roots of the plants being plunged in a little distilled water. The plants were then kept, alternately in sunshine and in the shade, for eight days, at the end of which time they were perfectly sound, and had grown considerably, but had not at all affected the purity or the volume of their atmosphere. In their green state, they now weighed 141 grains, and, after being dried in the common temperature of the atmosphere, their weight was only $40\frac{1}{2}$ grains. Similar plants, precisely of the same weight, were, at the same time, gathered and dried in the same manner, and were found to weigh $38\frac{1}{2}$ grains; so that those plants, which had grown eight days in confined air, exceeded them in weight, when both were reduced to the same state of dryness, only by two grains. These comparative experiments he repeated many times on different plants, and prolonged them to a fortnight or even a month; but he never found the weight of the confined plants to exceed that of others, which had not been confined, by more than two grains, and sometimes not at all, although they had, in some cases, grown several inches in length*.

519. To these experiments it may, perhaps, be objected, that the difficulty of reducing plants to a similar degree of dryness prevents us from arriving at any very accurate knowledge of their solid substance.

* Recherches, p. 221.

Though this, however, should be, in some degree, allowed, yet it must be granted, that the similarity of result, obtained in a great many trials, and the fact that no greater additional weight was acquired by the plants in a month than in eight days, although their bulk had then greatly increased, give great weight to the inference deduced from these experiments, since they shew that water, even if it be said to be decomposed, adds little or nothing to the solid vegetable substance.

520. But the great argument for the decomposition of water is drawn from the production of oxygen gas; and were it, therefore, to be said, that, in these plants, the two grains in weight were acquired by the addition of the hydrogen of the water, the plants, adds M. de Saussure, should have eliminated all the oxygen with which this quantity of hydrogen was combined, that is to say, at least 22 cubic inches, which should have been found in the recipient; whereas, it has been stated, that the atmosphere experienced no alteration whatever*. In the same manner, 19 cubic inches of oxygen should have been eliminated in an experiment made on some plants of *vinea minor*; but neither had their atmosphere undergone any sensible change†. If it be said, that the oxygen was converted into vegetable matter, as well as the hydrogen, then the argument for the decomposition of water, derived from the production of oxygen, necessarily falls to the ground; and as no evidence of this decomposition afterwards remains,

Recherches Chim., p. 222.

† Ibid. p. 224.

there is no just reason for supposing that it ever takes place.

521. As farther arguments against the decomposition of water, M. de Saussure observes, that he could never find in the atmosphere of confined plants, which grew many months in sunshine, any additional quantity of oxygen, which, however, might have been expected if they had directly decomposed water; but the air, in general, suffered no sensible change*. Whenever plants afforded oxygen gas in sunshine, it proceeded not from the decomposition of water, but of the carbonic acid contained in the substance of the leaf†; for when solution of potassa was placed in the vessel to attract the carbonic acid, the leaves then afforded no oxygen gas‡. From these and other facts, M. de Saussure concludes, that living plants do not, in any case, directly decompose water, by assimilating its hydrogen, and expelling its oxygen in a gaseous form; nor do they ever afford oxygen gas, but by the immediate decomposition of carbonic acid§. This conclusion seems to be fully warranted by the facts above stated, and perfectly accords with the opinion delivered (317.) in a former section.

522. But though the preceding experiments (518.) afford no support to the opinion that water is decomposed by growing vegetables, or even combined and fixed in them during vegetation, yet M. de Saussure believes this fixation to be prevented by the deficien-

* Recherches Chim. p. 230.

† Ibid. p. 233.

‡ Ibid. p. 235.

§ Ibid. p. 237.

cy of carbonic matter, deeming it probable that the quantity of oxygen and hydrogen in plants cannot be increased beyond certain limits, unless the carbon be supplied in proportion. To verify this idea, he refers to the experiments made on plants which grew in atmospheres of common air containing carbonic acid (310.), and in which that gas was decomposed, and oxygen gas expelled. He considers the carbon, which was previously united with this oxygen, to have been *assimilated* by the plant, and thereby to have augmented its vegetable substance. Thus the 21.75 cubic inches of carbonic acid, decomposed by the seven plants of *vinca minor*, afforded to the plants 4.2 grains of carbon. These plants weighed, in their green state, before the experiment, $168\frac{3}{4}$ grains, and contained 51 grains of dry vegetable matter; but, when reduced to dryness after the experiment, their weight amounted to 61 grains; so that, after thus decomposing the carbonic acid, their vegetable substance, it is said, was increased by ten grains. But of these ten grains, 4.2 only can be attributed to the carbon of the acid, and therefore the remaining weight, equal to 5.8 grains, must, says M. de Saussure, be ascribed to the fixation or solidification of water. Plants of *mentha aquatica*, in the same manner, augmented their dry vegetable substance by six grains, of which one half is ascribed to the assimilation of carbon, and the other half to the fixation of water*.

523. In opposition to this view of vegetable nutrition, we may observe, that etiolated plants, which

* Recherches Chim. p. 225.

decompose no carbonic acid, and therefore may be presumed to assimilate no carbon, contain, nevertheless, hydrogen, oxygen and carbon, as well as green plants in which this assimilated carbon is supposed to perform so important an office; so that, in such plants, at least, this supposed use of carbon, in promoting the fixation of water, is not necessary to vegetable nutrition and growth. Carbonic acid, also, seems to be decomposed in the cellular structure or parenchyme of the leaf, and not in its vascular system; and, for reasons already stated (468.), it appears to contribute rather to the augmentation of its resinous or colourable matter, than to its nutrition and growth. Admitting, therefore, that carbon is retained in the plant, there is no evidence of its *assimilation*, in the proper physiological sense of that term; for the resinous matter, which it contributes to form, is not a part of the vegetable structure, but a chemical product, which, according as light is either admitted or excluded, may, or may not, be formed or withdrawn, without injury to the organic structure of the plant.

524. In our apprehension of the term *assimilation*, it is that function or power, which living bodies possess, of converting and applying inorganic matter to their own support and increase, in a manner which has not yet been imitated by any mechanical or chemical means; and this function we believe to be performed, in the vascular system of plants, by a slow and successive series of changes, which the vegetable is able to carry on and accomplish, only while it possesses life. These changes, as far as regards the substances concerned, may be considered chemical; but,

in relation to the instruments by which they are performed, and the living power necessary to their execution, they do not fall to be considered as objects of that science. To conclude, that, because carbon is retained in the plant, it is at once assimilated and applied as food for its support, is to reduce the series of important changes, which terminates in vegetable nutrition, to one solitary chemical action, which we have seen to go on in circumstances and situations totally independent of the living powers and properties of the plant.

525. Neither because vegetables are chiefly composed of oxygen, hydrogen and carbon, and because water and carbonic acid are composed of the same elements, are we therefore entitled to suppose that these substances, by a chemical action upon each other, are, or can be, converted into vegetable matter, and disposed into that fine and varied structure which we call a plant. It is this structure itself which alone is capable of executing these changes; and the chemist can do no more than furnish the materials to carry them on, or, by varying the circumstances of the action, in some degree modify its result. The mere exertion of the affinities of bodies, be they what they may, can never compound an organic structure, whose distinctive character or essence depends, not on the nature of its substance or elements, but on the mode of their composition and arrangement, and on the properties which, as living matter, they acquire and possess. In physiology, therefore, it is necessary to consider, not merely the chemical nature of the bodies concerned, and of the combinations into which

they enter, but, likewise, the structure and properties of the instruments, by which these combinations are effected. It is, we believe, chiefly from inattention to these circumstances, that chemical hypotheses in physiology, though apparently consistent with chemical principles, have seldom received the approbation of those, who, from their knowledge of the structure and properties of living bodies, are, perhaps, best entitled to decide upon them; and, notwithstanding the confidence with which such hypotheses have been sometimes proposed, they have rarely extended beyond the confines of the laboratory in which they were formed.

526. Now that we have distinctly considered the effects which growing vegetables produce in the air, both when they are exposed to the sun, and when they are placed in the shade, it may, perhaps, be expected that we should add a few observations on the general question of the *purification* of the atmosphere by vegetation, to which so much importance has been attached. It appears, then, that oxygen gas is essential to vegetation; and that plants, at all times, both in sunshine and in the shade, convert it into carbonic acid. In sunshine, however, and probably, also, in some degree, in the shade, they possess the power of reconverting carbonic acid into oxygen gas; hence, therefore, the question, as to the absolute depravation or purification of the atmosphere by vegetation, must be decided by the difference in de-

gree in which one or other of these operations is found to prevail.

527. If, then, from what has now been stated, it appear, that living plants, at all times, and in all situations, require the presence of oxygen gas, and, during their growth, uniformly convert it into carbonic acid; and if it also appear, that the same plants, only at some times, and in some situations, decompose carbonic acid, and thereby afford oxygen gas to the atmosphere, it evidently follows, that, as the consumption of oxygen is uniform and general, it must exceed, in extent, its production, which is only occasional and partial. Add to this, that the plants which, during sunshine, are employed in producing oxygen, are, at the same time, engaged in consuming it; so that, even during that period, they may probably make no absolute addition of oxygen to the air; and, at other periods, they must, in common with other plants, directly deprave it. When, however, these two operations go on at the same time in sunshine, it is difficult to estimate the rate at which they respectively proceed, and, consequently, to determine the ratio which they bear to each other.

528. To the consumption of oxygen gas by plants which grow in the open atmosphere, no other limits appear to be placed than those which are afforded by the size of the plant, and the extent and vigour of its vegetative power; but the production of this gas by plants must depend on the supply of carbonic acid, and on the presence and efficiency of solar light. In consequence of the power which plants possess of simultaneously forming and decomposing (308.) car-

bonic acid in sunshine, we have seen; that, when confined for long periods in a given quantity of air, they produce in it little or no permanent change; but how far these two operations thus compensate each other in the open air, we possess not the means of deciding. To us, however, it appears, that, in the open atmosphere, vegetables can receive carbonic acid only, or chiefly, by means of their absorbed fluids; for the air that surrounds them, unlike that in closed vessels, scarcely contains any sensible portion of this acid, and, did it contain more, this gas would probably diffuse itself more readily through the atmosphere, than enter the substance of plants. In this view, therefore, although plants, in closed vessels, may maintain the atmosphere in a permanent state of purity, they may not be able to do so in the open air, from not receiving so abundant a supply of acid gas.

529. On the other hand, it appears, that carbonic acid is largely conveyed into plants, both in a state of simple solution, and of combination with alkaline matter; that these saline compounds are decomposed by light; and that the carbonic acid, at the same instant, is resolved into its constituent elements, so as to yield its oxygen to the air. These decompositions we have seen to be purely chemical, and to depend immediately on the presence and operation of light; hence their extent and rapidity will be bounded only by the supply of acid, and by the decomposing power which light is able to exert. Thus, in young and succulent plants, which possess abundance of saline matter, the green colour

rapidly forms, and carbonic acid is decomposed, so as to furnish oxygen to the atmosphere in a quantity that appears to exceed considerably that which the plant, by its vegetation, is, in the same time, able to consume. If, indeed, in certain circumstances, the decomposition of carbonic acid by plants did not exceed its formation, it is difficult to conceive how the composition of the air could be maintained unchanged in the experiments of M. de Saussure (305.); since the portion of time, in which the plants were exposed to the sun, was much less than that in which they were kept in the shade; and, even in sunshine, they were equally employed in forming carbonic acid, although, on many occasions, no trace of it could be discovered. They must, therefore, in the shorter period in which they were exposed to the sun, have decomposed not only all the acid which they had previously formed in the shade, but all that, also, which their vegetation produced while this decomposition was going on. These considerations, therefore, raise our view of the extent and importance of this decomposing power in plants; but, giving to them all the weight they deserve, we think, from the various facts and circumstances already stated, that growing vegetables deprave the atmosphere in a degree that greatly surpasses their power to ameliorate and improve it.

530. But if the operation of vegetables in purifying our atmosphere be, even under the most favourable circumstances, merely negative, and if, upon the whole, they must, like animals, be considered greatly to deprave it, where, it may be asked, are we to

look for those causes of purification, by the operation of which, the uniformity of composition in the atmosphere is, at all times, and in all situations, maintained? To this most interesting, but difficult question, no satisfactory answer can, we think, be returned, in the present state of chemical knowledge. The modes in which the atmosphere is depraved by the living functions of animals and vegetables, by combustion, and by various other processes, in which its oxygen is withdrawn and made to enter into new combinations, are pretty well known, and, to a certain extent, may be appreciated with tolerable accuracy; but the various means by which this oxygen is released from its combinations, in the diversified modes of decomposition which are perpetually taking place, have been much less regarded, and cannot, therefore, with equal accuracy, be traced. Until this department of chemistry attain to greater perfection, it is, therefore, impossible to present a tolerably accurate view of this subject. We may, however, be certain of the general fact, that, as oxygen is withdrawn from our atmosphere, in order to enter into new combinations, so it can again be restored to it only by decompositions which shall set it free; and these decompositions must be as numerous, and to an extent as great, as the combinations to which they succeed. To follow, however, this circle of actions through all its round, may demand the persevering industry of ages; and it is only when this shall be accomplished, that chemistry will have advanced our knowledge of the individual relations of our globe, in a degree corresponding with that to

which physical astronomy has carried its general connections with the universe,

531. But there have been writers, who rested their views of the purification of the atmosphere by vegetation, not so much on observation and experiment, as on what they conceived to be its necessity in the general economy of nature ; and, with more perhaps of piety than of prudence, and certainly with a “ zeal not according to knowledge,” they have represented the contrary doctrine as derogatory to the wisdom of Providence, and a calumny against Nature herself. It is indeed true, and it is among the most gratifying truths in the pursuit of science, that every real step which we make in the knowledge of nature, serves to illustrate the skill and wisdom with which all its parts are contrived to advance the general purposes of the whole ; but of this whole, it should also be recollected, that we, “ as yet, see but in part, and, as through a glass, darkly.” Hence imperfect and erroneous views of the order of nature may be often taken, and false conclusions may be grounded on them ; and if these conclusions be afterwards announced as examples of divine wisdom, and be allowed to borrow the authority of *final causes* for their support, the history of science abundantly testifies that the vainest conceits of fallible man may, in time, come to be worshipped as the wisest institutions of unerring nature. It behoves us, therefore, to employ no ordinary portion of delicacy and caution in pronouncing on the general plans and purposes of Providence, from the little and partial views of nature, which, at present, we are permitted to take, lest, in the effer-

vescence of our zeal, we degrade the wisdom we pretend to exalt, and pervert the designs of the goodness we profess to revere.

532. With respect, also, to the charge of calumniating nature, he, surely, who, by assiduous observation of the facts which she offers to his contemplation, seeks to discover the laws of their connection, and proposes his opinion of those laws as the simple result of his inquiries, may be regarded less as a calumniator, than he who supplies the imperfection and deficiency in his facts, by the suggestions of imagination, and confidently imposes upon nature laws and conditions, which she utterly disowns and disclaims. For ourselves, indeed, accustomed always to regard facts more than opinions, and to yield less deference to names than to things, we are little influenced by those speculations, which, in the language of Bacon, may be pronounced *anticipations*, rather than *interpretations*, of nature. Still less are we moved by considerations of the supposed consequences which others may attach to our opinions, but embrace the sublime admonition of an eminent and philosophical Professor,—“ To follow TRUTH wheresoever she may lead us, and, in all our researches, to be afraid of nothing but ERROR.”

ADDITIONS TO CHAP. III.

OF THE CHANGES INDUCED ON THE AIR BY THE
RESPIRATION OF ZOOPHYTES, WORMS, MOLLUSCA,
INSECTS, FISHES, AND REPTILES.

533. **I**N our former chapter on the respiration of the lower animals, we followed nearly the classification of the Linnæan school; but the farther we have advanced in our inquiries, the more have we perceived its imperfections. When treating simply of the changes induced on the air by the living functions of animals, we did not deem it necessary to enter minutely into the peculiarities of the animal structure, since it was our object, at that time, rather to ascertain the general facts with respect to the air, than to describe the organs by which respiration is performed, or the effects which it produces in the system. In the prosecution of this last branch of our subject, we have been obliged to enter on a more minute survey of the animal system; and it became, therefore, an object of importance to adopt an arrangement that should lead us through the chief va-

riations of structure with the least possible difficulty, and in the most natural order. Unfortunately, the Linnæan method is not well suited to this purpose; for being founded chiefly on an attention to external characters, with little or no regard to anatomical structure, it has, in many cases, associated together animals, whose general form and habits are essentially different, and in whom no common anatomical characters can with propriety be said to reside.

534. Since the time also of Linnæus, great additions have been made to this department of natural history; and the anatomical resemblances and differences in animals have been studied with a degree of zeal and success, unknown at any former period. Many improvements have, in consequence, been introduced into our systems of arrangement; and the French naturalists, who have laboured in this field with distinguished ability, have been induced, not merely to propose numerous alterations in the classes of animals, already established, but to institute other classes, which are entirely new. M. Cuvier has increased the number of classes, into which Linnæus divided the animal kingdom, from six to nine. This he has done, by forming into distinct classes, the Zoophytes and Mollusca, which ranked only as orders of the class Vermes in the arrangement of Linnæus. He has, likewise, formed into a class, under the title of Crustacea, the genera Cancer and Monoculus, which stand as an order in the Insect class of Linnæus. The writer of the article, "Classification," in the last edition of Dr Rees's Cyclopædia, has followed Cuvier in the two former altera-

tions ; but has replaced the two genera, Cancer and Monoculus, among the apterous insects. It is the arrangement of this author which we propose to follow in the remaining part of this work, because it appears to us to be the most natural, precise, and comprehensive ; and, being founded entirely on anatomical distinctions, it is best suited to display that progressive variation in the structure and functions of animals, which it will hereafter be our business to describe and explain.

535. Conformably to these views, we shall consider the lowest and most simple animals as forming a distinct class, under the appellation of Zoophytes. In many individuals of this class, the necessary concurrence of water and heat to the commencement of living action has been already exemplified (48. et seq.) ; and the same agents are essentially necessary to the development of the ova, from which other animals of this class proceed. Spallanzani removed the liquid from a number of the eggs of animalcules, so as to leave them perfectly dry, and they remained unchanged for ten days ; but on putting them into their native liquid, they were soon hatched *. So, likewise, he found, that, although great cold did not destroy the living faculty, in many animalcular ova, yet it suspended their evolution, which was always promoted by a due degree of heat.

536. With respect to the operation of air, the same author has remarked, that he never found any animalcule to be produced in vegetable infusions

* Tracts on Anim. and Veg, p. 66.

which were placed *in vacuo*, though the reverse uniformly happened, when the receiver contained a certain quantity of air. If, however, infusions, containing animalcules, were placed under the exhausted receiver, the animalcules lived many days, but perished long before others of the same kind, which were kept in the open air *. It may be doubted, however, whether the vacuum was in these experiments rendered perfect; for Dr Hooke found, that, when a quantity of vinegar, replete with eels, was included in a small phial, and stopped very close, all the included animals in a very short time died, as if they had been stifled †. The specific change effected in the air, by the respiration of this class of animals, may be inferred from the experiments of Mr Davy, who found (52.) that zoophytes not only require the presence of air in the water in which they live, but act upon it like fishes, that is, convert its oxygen gas into carbonic acid.

537. In the class Vermes, which falls next to be noticed in the order of our present arrangement, the first experiments, which we have met with, were made by the celebrated Scheele. He kept some leeches in a closed phial, which was half filled with water, and half with air, till they died. He then examined the air. It had no peculiar smell, but it extinguished the flame of a candle. These animals he found would live two years in the same water, when it had a free communication with the air; but

* Tracts, &c. p. 42.

† Micrographia, p. 217.

they died in two days, in a bottle quite filled with water, and closely stopped ; and the water, when examined, contained no oxygen gas *.

538. We have before detailed (55.) the accurate experiments of M. Vauquelin, on slugs and snails, which belong to the third or Mollusca class. He found that the oxygen gas of atmospheric air was completely changed into carbonic acid, without any alteration taking place in the whole bulk of air employed, or in the quality of its nitrogenous portion. Consequently, the bulk of acid gas produced must have exactly equalled that of oxygen gas lost, and no portion of oxygen could, therefore, have been retained in the animal system.

539. We have already endeavoured (56. et seq.) to point out the fallacies in Spallanzani's experiments, which seemed to militate against these conclusions. Of this writer's labours, three additional volumes, containing reports of several thousand experiments, have been since published by M. Senebier †. They do great credit to his industry, but, for accuracy and precision, they will not bear a comparison with those of the justly celebrated chemist, whom we before mentioned. In these experiments, the changes produced in the air by living animals, by animals recently dead, and by others under a state of putrefaction, form the chief subjects of investigation. In all cases, the oxygen gas of the surrounding air

* Treat. on Air and Fire, p. 167.

† Rapports de l'air avec les êtres organisés, tomes iii. à Genève, 1807.

more or less disappeared, and carbonic acid was, in a greater or less degree, produced. Sometimes the consumption of oxygen was complete, and an equal bulk of carbonic acid was produced ; at other times, the oxygen gas only partially disappeared, and the carbonic acid formed was in a much smaller proportion. Sometimes the nitrogen was in part consumed, at other times it remained unaltered, and in some instances its quantity was actually increased ; yet no attempt is made to investigate the causes of this discordance, or to discover the sources whence such great variations could proceed. Equal credit seems to have been attached to the most imperfect and the most perfect trials ; and the author appears, by the repetition of his experiments, to have simply added to their number without proportionally increasing their value. Of the labours of his predecessors and contemporaries he seems either to have been entirely ignorant, or to have maintained an injurious silence ; and hence he is sometimes found to announce well-known facts, with all the surprise and importance of new discoveries. Lastly, the effects produced in the air by living, by dead, and even by putrefying animals, are considered by him to proceed from the same causes, and to be accomplished in a similar manner ; and thus the results of decomposition are uniformly confounded with those which succeed to the exercise of living action.

540. From these various sources of inaccuracy, it is scarcely possible to deduce, from this author's experiments, any thing farther, with regard to respiration, than the general facts, that, in all animals, oxy-

gen gas is necessary to the continuance of living action ; that this gas disappears during the exercise of the respiratory function ; and that a portion of carbonic acid is, at the same time, produced : but the real nature of these changes in the air, the extent to which they proceed, and the mode in which they are accomplished, cannot be learned from the labours of Spallanzani. With all his zeal for experiment, and all his ingenuity, this philosopher, indeed, seems not clearly to have apprehended the true nature and objects of experimental science. The mere multiplication of experiments serves only to multiply error, unless the mind exercise a severe judgment over the observations of sense, and, by separating what is necessary from what is accidental, endeavour to trace the order and succession of events, and to ascertain the mode and degree in which they are finally accomplished. “ Inductio, enim, quæ procedit per enumerationem simplicem,” says the great Father of experimental science, “ res puerilis est, et precario concludit; et periculo exponitur ab instantia contradictoria, et plerumque secundum pauciora quam par est, et ex his tantummodo quæ præsto sunt, pronunciat. At *inductio*, quæ ad inventionem et demonstrationem scientiarum et artium erit utilis, naturam separare debet, per rejectiones et exclusiones debitas ; ac deinde post negativas tot quot sufficiunt, super affirmativas concludere *.” Even when this separation and rejection shall be made, the facts thus acquired

* Nov. Organ. lib. i.

will still remain barren and unproductive, if not quickened into life by the pervading energy of the mind. This "*commercium mentis et rerum*," it was, indeed, the great aim of Lord Bacon's philosophy to promote. "For thoughts," says one of his earliest disciples, "cannot work upon nothing, no more than hands. He that would build a house, must provide materials; and, on the contrary, the materials will never become a house, unless, by certain rules, we join them all together. So, it is not simply the knowledge of many things, but a multifarious copulation of them in the mind, that becomes prolific of farther knowledge *."

541. Concerning the respiration of insects, we before remarked (51.), that the ancients observed these animals to die when their bodies were smeared over with oil, which effect Mr Ray rightly attributed to the "intercluding of the air." Mr Boyle, among the almost innumerable experiments which he made with the air-pump, found, that flies, bees, and butterflies died in a few minutes when the receiver was exhausted of its air. It was notable also, he adds, that though bees and flies will walk and fly a great while after their heads are cut off, or even one half of the body move several hours after being severed from the other, yet, upon the exsuction of the air, not only the motion of the body, but of the limbs, ceases, as if the presence of air were more necessary than that of their own heads †.

* Grew's Anat. of Plants, p. 8.

† Boyle's Works, 4to, vol. i. p. 79. 112.

542. In our former work, we ascribed the first discovery of the specific changes which insects induce on the air to M. Vauquelin ; but we have since found, that it ought rather to have been given to Scheele. He shut up several flies in a phial, and in a few days they all died, but had not diminished the volume of air. Milk of lime, however, reduced the bulk of residual air one-fourth part, and the remaining air extinguished a taper. He likewise inclosed a bee in a bottle, containing 20 ounce measures of air. A small piece of lime was, at the same time, introduced into the bottle, and in its side, near the bottom, a small hole was made. The bottle was then immersed in water, and as the water passed in, a quantity of lime-water was formed, which attracted the carbonic acid produced by the insects. As by this attraction of acid the volume of air continued to diminish, the water also continued to enter through the hole, until, by the seventh day, it had risen to about one-fourth of the height of the bottle. If two bees were put into the bottle, the same quantity of air was changed into carbonic acid in half the time ; and he found also, that caterpillars and butterflies produced similar effects in the air.

543. To prove that it was the oxygenous portion of the air that was thus changed into carbonic acid, he confined two large bees in the same bottle, when it was filled with oxygen gas ; and then set it to stand in milk of lime. He observed every day the milk of lime to rise through the hole into the bottle ; and by the eighth day, the vessel was nearly filled

with it, and the bees were dead *. No experiments can more clearly shew the necessity of oxygen gas to the life of insects; nor more completely demonstrate its almost entire conversion into carbonic acid.

544. The ova of insects, not less than the perfect animals, require the combined operation of water, heat, and air, to carry forward their evolution. Many of these ova are deposited in water, and are evolved only during their immersion in that fluid; and the influence of heat is strikingly exemplified by the fact, that it is only during the warmer part of the year that the young broods of insects appear. That air, also, is necessary, may be concluded from the experiments of Spallanzani, who placed the eggs both of terrestrial and aquatic insects under the exhausted receiver of an air-pump, but none of them were hatched, although every other condition, necessary to their development, was present †. He found, also, that the ova of various insects were evolved in large vessels, even when the vessels were hermetically sealed; but, in proportion as the size of the vessels was diminished, the progress of evolution was retarded, and when the volume of air was reduced to a few inches, no signs of evolution then took place ‡.

545. The same observations may be extended to the intermediate, or *larva* state of insects. Spallanzani ascertained that these larvæ soon died when confined *in vacuo*, or in close vessels of air, though

* On Air and Fire, p. 148. 155.

† Opuscles de Phys. t. i. p. 141. ‡ Ibid. t. ii. p. 251.

abundantly supplied with food *. He discovered also, that they soon perished in nitrogen and hydrogen gases; that oxygen gas was necessary to support their life, and was consumed by their respiration; and that a quantity of carbonic acid was produced. The cabbage and silk-worm caterpillars consumed, more or less completely, the oxygen gas of the air, and produced carbonic acid. The more vigorous and active they were, the more oxygen they consumed. In a high temperature, also, they consumed more than in a low one; and when the temperature fell to *zero*, they then produced no change whatever in the air employed †. He found, likewise, the larvæ of the dragon-fly, and of other aquatic insects, to require the presence of oxygen, in the water in which they lived, to consume it by the exercise of their living functions, and to form carbonic acid ‡.

546. If, farther, we follow the *larva* to its *chrysalis* or *pupa* state, it will be found still to require the presence of the same agents, to enable it to go through its series of successive changes. The influence of heat, in carrying forward these changes, is well illustrated by some experiments of M. de Reaumur. That celebrated naturalist placed the pupæ of many species of insects in a green house, and the butterflies appeared in the middle of winter, some in ten or twelve days, others in three weeks, and others at a later period, corresponding to the earlier or later periods at which they naturally appear in the sum-

* Tracts, &c. p. 227.

† Rapports de l'air, &c. t. i. p. 17. et seq.

‡ Ibid. p. 72.

mer months. All of them, however, changed much more rapidly, from the permanent continuance of the heat, than if they had been exposed, even in summer, to the variable temperature of the open air. The insects, thus prematurely produced, were perfect in all their parts, and the females laid their eggs as usual ; so that, by this method, butterflies may be made capable of giving birth to two generations in the year, though in ordinary circumstances they afford but one. The same results were obtained by experiments made on the pupæ of various other insects.

547. M. de Reaumur, likewise, confined several pupæ in a hollow sphere of glass, and placed them, with the eggs, under a brood-hen, leaving an aperture in the glass, by which fresh air could enter. The next day, the interior sides of the glass were covered with vapour that issued from the pupæ, and had collected into small drops of water. These were removed, and did not appear afterwards, the greatest transpiration having occurred during the first day. At the end of four days, he saw, as he observes, the first butterfly that had, perhaps, ever been born under a hen, and the first of this species of pupa that had ever remained so short a time in that state ; for others of the same species, in the open air, require from twelve to sixteen days to complete the same series of changes*.

548. On the other hand, by keeping the pupæ of insects in a vault, where the temperature was only 8 or 10° Reaum. above the freezing point, their e-

* Mem. pour l'hist. des Insectes, tom. ii. p. 8. et seq.

volution was retarded in the summer season, and, instead of being changed into butterflies in July, they still retained their pupa state in the month of November. In this manner, they were kept more than a year in their pupa state, and this mode of existence might probably be prolonged for many years, without affecting their power of reviviscence, especially if they were kept in an ice-house, where the temperature did not rise above *zero* *.

549. That air is necessary to the development of these animals, may be inferred from the experiments of the same author. In July, Reaumur confined the pupæ of butterflies and moths in glass tubes four or five inches long, which were hermetically sealed; and, in these circumstances, they remained in their original state for more than five months, without indicating any appearance of development †. But when Spallanzani kept the pupæ of silk-worms in larger tubes, which were likewise sealed, their evolution proceeded, and they attained their perfect insect state. Similar observations were made on the pupæ of flies and butterflies ‡; and we may therefore conclude, that fresh air is necessary to carry forward the change of the insect from its pupa to its fly state. By subsequent experiments, Spallanzani also found, that the oxygen gas of the air was consumed in the transformation of the pupa to the insect; that carbonic acid was produced, but that

* Mem. &c. tom. ii. p. 18.

† Mem. tom. ii. p. 6.

‡ Tracts, &c. p. 200. 248.

the nitrogenous portion of the air remained unaltered. In temperatures at or below zero, no change however was produced in the quality of the air *. We repeated these experiments on the pupæ of the common fly, and found, that, during their transformation, they consumed all the oxygen gas of the air in which they were confined, and produced an equal volume of carbonic acid.

550. It has been already shewn (63.), that Fishes require the presence of oxygen gas in the water in which they live, and convert it into carbonic acid. These facts have been confirmed by the experiments of M. Sylvestre †, and by those of Spallanzani. The latter philosopher ascertained, by experiment, that fishes consume oxygen gas and produce carbonic acid, when confined either in water or in air; that they lose their vivacity, but do not become lethargic under cold, and then consume much less oxygen; and that oxygen gas is changed into carbonic acid by their skins, as well as by their gills. Their bodies, also, act upon the air after death, and, like other animal substances, consume its oxygen under decomposition ‡.

551. We are indebted to M. M. Humboldt and Provençal, for experiments on the respiration of fishes, which possess still greater accuracy. Their

* Rappports, &c. tom. i. p. 44. 50.

† Cuvier's Leçons d'Anat. comp. tom. iv. p. 305.

‡ Rappports, &c. t. i. p. 186.

attention was first directed to ascertain the quantity and composition of the air that exists naturally in river water. For this purpose, they filled glass-balloons with given quantities of water, taken from the river Seine, and expelled the air from it by submitting it to ebullition. The air that came over was received in vessels filled with mercury, or with distilled water, recently boiled, that no foreign air might mix with that obtained from the water in the balloon. From the results of ten experiments, conducted in this manner, they found, that the water of the Seine contained rather less than $\frac{1}{30}$ of its volume of air. This air they farther found to be composed of about $\frac{31}{100}$ oxygen, with from six to eleven *per cent.* carbonic acid, and the remainder was nitrogen gas. The composition of the air, contained in river-water, they state to be as constant in its proportions as that of the atmosphere; for in experiments continued many months, in times of drought and during the melting of snow, the proportion of oxygen in the water of the Seine never varied more than from 0.309 to 0.314 *. This estimation of the quantity and composition of the air, contained in river-water, does not differ so much as might have been expected from that afforded by the water of springs; for, according to Mr Dalton, air expelled from common spring water, after losing from five to ten *per cent.* carbonic acid, consists of 38 *per cent.* of oxygen and 62 of nitrogen gas†.

552. Having thus determined the quantity and kind of air contained in a given volume of river-wa-

* Mem. de d'Arcueil, tom. ii. p. 367. et seq.

† Chem. Phil. part ii. p. 272.

ter, these chemists proceeded to ascertain the changes which it experienced by the respiration of fishes. With this view, they confined young fishes in bell-glasses of river-water, inverted over mercury ; and suffered them to remain till their respiration became laborious. The animals were then withdrawn, and the water, in which they had respired, was transferred into the balloon, and its air expelled, by submitting it to ebullition, in the manner before stated. Seven tenches were, in this manner, confined in 4000 cubic centimeters, equal to 250.5 cubic inches of river-water, where they remained eight hours and a half. A portion of this water, equal to 2582 cubic centimeters, or 161.5 cubic inches, was then transferred from the glass-bell into the balloon, and its air expelled by heat. The air, thus obtained, measured 453 parts, at temperature 50° Fahrenheit. These 453 parts were then washed in lime water, by which they were reduced to 300, so that 153 parts of carbonic acid were thus removed. The residue was afterwards analysed by combustion with hydrogen, and by mixture with nitrous gas ; and the mean of three analyses afforded 0.035 of oxygen ; wherefore it is concluded, that the 453 parts of air, obtained from water which had been in contact with the respiratory organs of fishes, consisted of 10.5 oxygen, 289.5 nitrogen, and 153.0 carbonic acid gas. But by former experiments, it was found, that an equal volume of pure river-water afforded 524 parts of air, consisting of 155.9 oxygen, 347.1 nitrogen, and 21.0 carbonic acid ; consequently, say these chemists, these seven tenches have *absorbed*, in eight hours, 145.4 of oxygen, and 57.6 of nitrogen gas ; and they have pro-

duced, in the same time, 132 parts of carbonic acid *.

553. The fact of the disappearance of a portion of the air in this experiment we do not dispute; neither do we question the accuracy of the analysis of the air which was actually obtained; but to the notion that the air, which disappeared, was *absorbed* by the animal system, we cannot so readily accede. No reasons, either anatomical or chemical, are assigned in support of such an opinion; neither is any evidence offered of the existence of these gases in the animal system, nor of their subsequent expulsion from it. The mere loss of a part of the oxygen seems to have afforded sufficient proof that the whole of it had first been absorbed; and that the portion of it, not returned in the form of carbonic acid, was permanently retained in the system. We forbear to repeat the general arguments against an absorption, or attraction of air by the gills of fishes, which have been already stated (74.5), but shall notice a few sources of fallacy in these experiments, which may, perhaps, explain the manner in which a part of the air might disappear, without resorting to the improbable supposition of its absorption.

554. The means of ascertaining with absolute precision, the volumes of air before and after respiration, have always constituted the great difficulty in experiments on this function, even in terrestrial animals; but as the sources of fallacy have been successively detected and removed, the correspondence between the

* Mem. d'Arcueil, tom. ii. p. 376.

inspired and expired volumes of air has increased, so as, in most cases, to have reached, at length, an almost perfect equality. If, however, these difficulties exist, when we operate with air alone, how much more may they be expected to attend experiments on air, dissolved in water? In the experiments which have now been detailed, it may be said, that, if the same degree of ebullition were kept up, the same quantity of air might be expected to be obtained from the water, after the fishes had breathed in it, as before, provided this quantity really existed in it; but as, by the act of respiration, the composition of the air is changed, this circumstance may, perhaps, vary the quantity retained by the distilled water through which it is subsequently passed, and thus occasion a variation in the result. Farther, in the act of transferring the water from the glass-bell into the balloon, a considerable portion of the acid gas might escape; for, according to Scheele, the carbonic acid, formed by aquatic animals, must always separate from the water, since, as carbonic acid, it does not remain with water in the open air* ;—a fact which is evinced by the small portion of carbonic acid contained in river-water, notwithstanding the great quantities that must be constantly forming in it both by the living functions of animals, and by the decomposition of dead organised matter.

555. That these, or other sources of fallacy, exist in these experiments, may be farther assumed from

* On Air and Fire, p. 165.

the great variations in the results which they afford. In a table, which presents the results of seven different experiments, conducted in a similar manner, the consumption of oxygen to the carbonic acid produced, is represented, in one instance, as bearing the proportion of 100 to 91 ; in another, the proportion is as 100 to 50 ; and in a third experiment, as 100 to 20 * ; so that the same animals appear, at one time, to have consumed only one-tenth ; at another time, one-half ; and, at a third time, four-fifths of the oxygen employed ;—contrarieties which sufficiently demonstrate the existence of some undiscovered sources of error. These experiments, therefore, can be considered as proving only the general fact of the conversion of oxygen gas into carbonic acid by the respiration of fishes ; but they do not enable us to determine the actual extent to which this conversion proceeds. As, however, in the experiment that has been detailed (552.), nine-tenths of the oxygen, which disappeared, were obtained in the form of carbonic gas, it is, surely, more reasonable to suppose that the remaining one-tenth was converted into the same gas, than that it entered into, and was retained in the animal system. On this point we may be permitted to call in the aid of analogy ; for as, in the respiration of every other class of animals, the bulk of acid produced is proved to be nearly or exactly equal to that of oxygen consumed, it may be fairly inferred that a similar equality obtains in the respiration of fishes, to which, indeed, the volumes of air in

* Mem. d'Arcueil, tom. ii. p. 378.

the foregoing experiment make a near approximation.

556. In like manner, from the same table, we learn, that, in some experiments, the loss of nitrogen amounted to about $\frac{1}{12}$; in other instances, to one-sixth; and in one case, it extended to about $\frac{1}{3.5}$ of that gas. This disappearance of nitrogen is, likewise, in every instance, ascribed to *absorption*; and the circumstance is pointed out as constituting an important difference between the respiration of fishes, and of the mammalia, in whom no absorption of nitrogen is conceived to take place. No other evidence, however, of this absorption of nitrogen is afforded, than the mere disappearance of a portion of that gas; and all the differences in the results are at once ascribed to variation in the exercise of this supposed absorbing power. In any investigation purely chemical, we are persuaded that similar discrepancies would have led to some attempts to discover their cause; but, in the application of chemistry to physiology, the most contradictory results obtain equal credit, and all the anomalies which may arise are at once charged, not to errors in experiment, but to some unknown operation of the animal system, which is called into existence for the mere purpose of carrying on some other operation, that is equally improbable and unknown.

557. As the air dissolved in water does not exceed $\frac{1}{36}$ of the volume of that liquid, and only $\frac{3.1}{100}$ of this air are pure oxygen, the relation of fishes to the oxygen contained in water corresponds with that of an animal breathing in a gaseous mixture, which

contains less than $\frac{1}{80}$ of that gas. This small quantity of oxygen gas in water might lead to the belief that it is not so necessary to the life of fishes as of other animals; but these animals soon suffer from the smallest suspension of their respiration; and this distress seems to arise more from the deficiency of oxygen, than from the presence of carbonic acid *. For when water is completely deprived of its oxygen, fishes die in it in a few minutes; but if oxygen be present, they live very well in water that contains more than one-eighth of its volume of carbonic acid †.

558. To maintain the respirability of the air dissolved in water, ample means are provided. Dr Priestley long since remarked, that boiled water decomposed air by attracting its oxygen ‡. And Scheele, also, observed, that water attracted oxygen, but not nitrogen ||. M. de Marty, however, observes, that nitrogen is attracted by water in limited quantity, and that it afterwards attracts oxygen, so as accurately to analyse atmospheric air. To saturate water completely with air, exposure to the atmosphere seems to be necessary; for M. Humboldt remarks, that water, which has been deprived of air, does not recover either the same volume of air, or the same proportions of oxygen and nitrogen under close vessels, as when it is exposed to the free atmosphere §. From

* Mem. d'Arcueil, t. ii. p. 379. † Ibid. p. 380.

‡ Phil. Trans. vol. lxii. p. 247.

|| On Air and Fire, p. 163.

§ Mem. d'Arcueil, tom. ii. p. 370.

this superior attractive power which water possesses for oxygen, the air dissolved in it is found to contain always a due proportion of that gas. But the experiments of Scheele (554.) also shew, that, as the oxygen is thus attracted, the carbonic acid is expelled, so that that gas never exceeds a certain quantity in the water, either of springs or of rivers. In like manner, Dr Priestley observes, that hydrogen, during its solution in water, seems to expel nitrogen, while nitrogen and oxygen are capable of existing together in that fluid *. By this difference in attractive power which water possesses for the different gases, connected with the expulsive force which they seem to exert towards each other, the noxious gases, formed in water by the exercise of the animal functions, and by the decomposition of organic bodies, are regularly expelled ; and thus the air, destined to support the living functions of aquatic animals, like that of the atmosphere which we breathe, is maintained nearly in an uniform state of composition and purity.

559. The last class of inferior animals, in the arrangement which we have now followed, is denominated Reptiles, in which are included all the animals that formed the Linnæan class of Amphibia. The experiments already given (60 et seq.), clearly prove, that frogs and toads, which belong to this class, entirely convert, by respiration, the oxygen gas of the air into nearly an equal bulk of carbonic acid, without producing any change in its nitrogenous portion.

* Obs. on Air, vol. i. p. 59.

Dr Carradori, also, discovered, that these animals lived much longer when they were immersed in water that had a free communication with the atmosphere, than when the air was excluded*. According to Spallanzani, frogs die sooner in boiled, than in common water. In their respiration, they consume oxygen, and form carbonic acid. Those which have been recently fed, consume more of this gas than those which have suffered a long abstinence. Under great cold, they become lethargic, but their heart still continues to beat, and they still, in a smaller degree, continue to change the air; but the consumption of oxygen increases with an increase of temperature. These animals also change the air by their skin, as well as by their lungs; and act upon it after death, and under putrefaction†. The ova of frogs were likewise found to require air to carry on their evolution. Small tadpoles, while yet attached to the egg, were confined in vessels half filled with water, while the other half contained common air, or oxygen, or nitrogen gas. Those in the two former vessels were perfectly developed, and became large enough to swim about; but those confined with nitrogen perished‡.

560. Spallanzani extended his experiments to many other animals of this class, and obtained similar results. Different species of serpents he found to die in hydrogen gas, or when confined under water, but

* Phil. Magaz. vol. xvi. p. 245.

† Rapports, &c. tom. i. p. 468.

‡ Ibid. p. 466.

to live in common air, and convert its oxygenous portion into carbonic acid. They became lethargic from cold, and the heart then beat very slowly or not at all; the respiration was then also suspended, and little or no effect was produced in the air. The skin of these animals acted upon the air as well as the lungs *; and when their blood was reddened by exposure to the air, its oxygen also disappeared, and carbonic acid was produced †. Similar results were obtained in experiments on the respiration of vipers, tortoises, lizards, and salamanders ‡.

561. The preceding facts sufficiently shew, that various animals, in all the foregoing classes, and in every stage and form of their existence, require the presence of oxygen gas to maintain the functions of life; that this gas, by the exercise of these functions, is converted into carbonic acid; and that the degree in which this conversion proceeds, depends much on the healthy condition of the animal, and the vigour of its circulating system. Since, also, in every instance where the experiments have been made with the requisite accuracy, the bulk of carbonic acid produced, nearly or exactly equalled that of the oxygen which disappeared, we may conclude, from analogy, that such is universally the extent to which this change in the air takes place in animal respiration; and since, farther, the nitrogen gas of the air appears to suffer no necessary change (73.) in the exercise of this function, we may also conclude, that, as far as

* Rappports, &c. tom. i. p. 249. † Ibid. p. 239. 263.

‡ Ibid. p. 275. 287. 295. 353.

regards the air, the substitution of an equal bulk of carbonic acid for the oxygen gas that is lost, comprises the only essential change which the atmosphere experiences during the performance of this animal process. We have before maintained (74 et seq.), that the oxygen of the air does not enter the animal system, either by the living function of absorption, or by the operation of chemical affinity; and have consequently concluded, that the union of this substance with the animal carbon takes place exterior to the vessels of the living animal.

ADDITIONS TO CHAP. IV.

OF THE CHANGES INDUCED ON THE AIR BY THE
RESPIRATION OF BIRDS, OF QUADRUPEDS, AND
OF MAN.

562. **W**E before pointed out (79. et seq.) the general agency of water and heat in contributing to produce the phenomena of living action in the superior animals; and from the writings of Boyle and others, we adduced facts to shew the constant necessity of fresh air to maintain the functions of life. This necessity Mayhow first proved to arise from the consumption of what he called the nitro-aërial or pure part of the air in respiration, which wholesome part was shewn by Dr Black to be converted into carbonic acid. By his discovery of oxygen gas, Dr Priestley was enabled to demonstrate the specific portion of the air which was thus changed *; and M.

* It should be mentioned, in justice to the celebrated Scheele, that he, likewise, discovered oxygen gas independently of Dr Priestley, as, in the following paragraph, the Doctor himself, with

Lavoisier, by ascertaining the true composition of carbonic acid, enabled us to determine in what way the acid gas, obtained in respiration, might be formed. Such is the order in which the facts, relating to our knowledge of the properties and uses of the air in respiration, have been successively disclosed.

563. But we farther endeavoured to shew, that the actual quantity of carbonic acid, formed in respiration, corresponded very nearly (122.) with that of oxygen gas which had disappeared; and that, allowing for a small degree of *condensation*, which we then supposed to attend the conversion of these gases into each other, “the whole of the oxygen gas lost (123.), was employed to form the carbonic acid in question.” This conclusion, we maintained, rested not only on

his usual candour, admits. “Having made this discovery”, says he, “some time before I was in Paris in 1774, I mentioned it at the table of M. Lavoisier, when most of the philosophical people in the city were present, saying that it was a kind of air in which a candle burned much better than in common air; but I had not then given it any name. At this, all the company, and Mr and Madame Lavoisier as much as any, expressed great surprise. I told them that I had gotten it from *precipitate per se*, and also from red lead. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plomb rouge*, which was not understood till M. Macquer said I must mean *minium*. Mr Scheele’s discovery was certainly independent of mine, though, I believe, not made quite so early*.” Yet, when speaking of oxygen gas, M. Lavoisier observes, “This species of air was discovered, almost at the same time, by Dr Priestley, Mr Scheele and myself†.”

* Treat. on Phlogiston, p. 56. an. 1800.

† Elem. Chem. p. 84. 4th edit.

the most correct experiments which had then been made, but was supported by the whole range of analogical facts, presented in the vegetation of plants, and in the respiration of the inferior animals; and it is a conclusion of such great importance, in conducting us towards a true explanation of the respiratory function, that we shall not hesitate again to recite, very briefly, the evidence adduced in its support, together with such additional facts and arguments, as subsequent researches have enabled us to collect and bring forward.

564. Among his earliest experiments relating to respiration, Dr Priestley found, that if a mouse was confined in a jar of air, inverted in mercury, until he died, no diminution in the bulk of air took place, but the residual air lost nearly one-fifth of its bulk, when shaken with water *;—facts which entitle us to infer, that no portion of the air was absolutely lost, but that the whole of its oxygen was changed into an equal bulk of carbonic acid. Similar results, as we before remarked (83.), were obtained by Dr Crawford, who also observed no diminution to attend the respiration of air inverted over mercury, but the residual air afterwards lost one-fifth of its bulk by agitation with solution of potassa. Dr Menzies, in his experiments on respiration, found no diminution to occur in the volume of air respired; and therefore necessarily inferred that the bulk of acid formed was equal to that of oxygen gas which disappeared †.

* Phil. Trans. an. 1772.

† Dissert. on Respirat. p. 50.

565. In the year 1806 Mr Dalton's attention was directed to this subject, and he satisfied himself, by numerous experiments, that the bulk of carbonic acid, formed in respiration, was exactly equal to that of the oxygen gas consumed. On repeating these experiments, Dr Thomson obtained, in some cases, nearly the same results; but, upon the whole, the bulk of oxygen that disappeared was somewhat greater than that of the carbonic acid formed. The difference, however, varied considerably, and kept pace with the diminution in the whole bulk of air; whence he considers it to arise from the abstraction of a part of the air by some other way than by respiration; and if this be allowed for, he believes "the bulk of acid produced to be precisely equal to that of oxygen gas lost". Hence, says he, this oxygen must be changed into carbonic acid in the lungs; for oxygen gas, when changed into carbonic acid, does not sensibly alter its bulk †.

566. These conclusions have been since confirmed by some very accurate experiments of Messrs Allen and Pepys. They caused a person to inspire, from a gasometer, 3460 cubic inches of atmospheric air, which were afterwards expired into another gasometer; and to both gasometers graduated scales were affixed, by which the quantities of air received and expelled could be accurately measured. The time occupied in the experiment was eleven minutes: about 58 respirations were made; and the deficiency in the whole volume of air, at the close of the expe-

* Syst. Chem. vol. v. p. 736. 3d edit.

† Ibid. p. 774.

riment, amounted only to 23 cubic inches. One hundred parts of the respired air afforded, on analysis, 8.5 carbonic acid, 12.5 oxygen, and 79 nitrogen gas*. The experiment was repeated several times; and in one instance, 9890 cubic inches of air were breathed for $24\frac{1}{2}$ minutes, with the loss of only 18 cubic inches, and 100 parts of the expired air then afforded, on analysis, 8 carbonic acid, 13 oxygen, and 79 nitrogen †.

567. Now the air employed in these experiments contained, in 100 parts, 21 oxygen and 79 nitrogen; and, in the numerous analyses which were made of this air after its respiration, the portion of oxygen that disappeared was exactly replaced by that of carbonic acid produced; so that, in every instance, these two gases formed together $\frac{21}{100}$ of the respired air, the remaining 79 parts being pure nitrogen gas. It is, therefore, concluded, “that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed ‡.”

568. In subsequent experiments on the respiration of a Guinea pig, these chemists found, that when 310 cubic inches of atmospheric air were breathed for 25 minutes by this animal, its volume experienced no variation whatever, and the portion of its oxygen, which disappeared, was replaced by an equal bulk of carbonic acid §; wherefore, they justly conclude, “that when atmospheric air alone is respired, even

* Phil. Trans. 1808, p. 254.

† Ibid. p. 257. ‡ lb. p. 279.

§ Ibid. 1809, p. 414.

by an animal subsisting wholly upon vegetables, no other change takes place in it than the substitution of a certain portion of carbonic acid gas for an equal volume of oxygen *."

569. It is, we conceive, impossible to refuse entire credit to the accuracy of these experiments, or to the legitimacy of the conclusions drawn from them, so far as relates to the chemical changes actually induced on the air; and these conclusions we have seen, also, to be supported by the experiments of many other able chemists, and by the results afforded in the exercise of the respiratory function in almost every other class of animals. We grant, however, that other chemists, equally able, whose experiments we have already detailed (122.) have concluded, that, because the carbonic acid obtained did not quite equal in bulk the oxygen lost, a portion of this latter gas was *absorbed* in the lungs. In some late experiments on the respiration of rabbits, and of Guinea pigs, M. Berthollet adopts this opinion. In these experiments, the bulk of acid gas produced did not quite equal that of oxygen which disappeared, so that the loss of oxygen appeared to vary from 1.07 to 4.09 *per cent* †. These two results, therefore, differ more from each other, than the former does from that of Allen and Pepys; and as this difference may be conceived to indicate error in that experiment which afforded the least acid, it may reasonably be inferred, that, had greater correctness been attained, no

* Phil. Trans. 1809, p. 427.

† Mem. d'Arcueil, t. ii. p. 461.

loss whatever would have appeared in that which yielded the most. In experiments, where so many causes concur to render the apparent bulk of acid less than it ought to be, and less than that of the oxygen lost, it is surely more reasonable to give greater credit to those results which indicate an equality of volume between these gases, than to those which declare a difference; since the former not only go with the latter to the fullest extent, but, pursuing the same track, have actually gone beyond them, and thereby reached a point, which the others have been unable to gain. In fact, to prefer those experiments, which indicate a difference, to those which prove an equality of volume, would be not only to halt in our progress, but to make a retrograde movement, and thus to suffer a negative inference to outweigh a positive proof.

570. Even Messrs Allen and Pepys, however, whose experiments so clearly demonstrate the equality of bulk in these two gases, in every case of natural respiration, have been led, by the results of other experiments, to suppose, that, “when respiration is attended with distressing circumstances, there is reason to conclude that a portion of oxygen is absorbed *.” In the deduction of such an inference, regard seems to have been paid to mechanical and chemical considerations alone, and little or no attention given to the structure and properties of the living instruments by which respiration is performed. We before endeavoured to impress the necessity of at-

* Phil. Trans. 1808, p. 280.

tending to the natural actions of the respiratory organs (92. et seq.), in the conduct of all experiments made on the exercise of this function, by shewing, that, in all examples of natural respiration, little or no variation occurred in the volume of air employed, but that, in proportion as these organs suffered distress and oppression, the greatest irregularities prevailed.

571. These facts are supported, in all their circumstances, by the results obtained by Messrs Allen and Pepys. We have seen (568.), that, when a Guinea pig was made to breathe a given quantity of air in a natural manner, no variation whatever was observed in its bulk; and even in man, in whom many causes, which do not affect the lower animals, contribute to produce error, it appears, that, when the respiration was nearly natural, the general average of the deficiency, in the total amount of common air inspired, was only about six parts in 1000 (566.); and in one instance, it was considerably less than two. "The smallness of this deficiency," say these chemists, "surprised us very much; and, in our opinion, it principally arises from the difficulty, or," as they elsewhere say, "the impossibility of always bringing the lungs to the same state after forcible expiration *."

572. Notwithstanding the justice of this remark, they seem, however, entirely to have neglected its import in the inference which they have drawn from their fourteenth and fifteenth experiments. In the first of these experiments, 300 cubic inches of atmos-

* Phil. Trans. 1808, p. 253 254-258.

pheric air were, in the space of three minutes, passed eight or ten times through the lungs, until respiration became extremely laborious, and the operator was compelled to desist. On analysing the respired air, it was found to contain, in 100 parts, only 5.5 oxygen, 9.5 carbonic acid, and 85 parts of nitrogen gas. In the fifteenth experiment, which occupied, also, about three minutes, until the operator became quite insensible, the same quantity of air was employed, and afforded, by analysis, nearly the same results; for it contained, in 100 parts, four of oxygen, ten of carbonic acid, and 86 of nitrogen. In the former experiment, we observe, therefore, an increase of six parts of nitrogen, and a loss of six parts of oxygen; and in the latter, the oxygen had lost 7 from 21, and the nitrogen had gained 7 upon 29*; and hence it is inferred, that “when,” as in these experiments, “respiration is attended with distressing circumstances, there is reason to conclude, that a portion of oxygen is absorbed †.”

573. To this inference, as far as it regards what is here called an *absorption* of oxygen, we must beg leave to object. That the united volumes of oxygen and carbonic acid expired, were less than the total volume of oxygen inspired, we readily grant; but we deny that this fact affords any adequate proof of an absorption of this latter gas. To the chemist, indeed, the mere fact of the disappearance of a portion of oxygen may supply sufficient evidence of its absorption, in the sense in which he may choose to em-

* Phil. Trans. 1803, p. 260.

† Ibid. p. 280.

ploy that term ; but the physiologist farther requires to know, by what organs or vessels it is removed, in what course it is conveyed, and what uses it is destined to serve. On none of these points, however, does he gain any information ; and all the anatomical knowledge which he possesses of the structure of the lungs, and of the properties of the living absorbent system, is adverse to such a doctrine. Should he apply to the chemist for a solution of his difficulties, he is told that oxygen does not chemically combine with other bodies, unless it be brought into actual contact with them ; and he knows, that, in the present case, this contact is impossible, because the membranes, both of the air-cells and blood-vessels, are interposed between the air and the blood in the lungs. Even if, contrary to all experience and analogy, he were to concede to the chemist the existence of pores or other passages in the cells and blood-vessels, through which this oxygen might be attracted and combine with the blood, he is equally embarrassed to discover the reason or mode in which it is again so speedily expelled, or what useful purpose it can serve, since no portion of it is permanently retained. The science of chemistry furnishes no example of similar operations,—of fluids which attract gases and combine them, so as to reduce their elasticity, and then, without any apparent change of condition or circumstances, almost instantly discharge them in a new and elastic form.

574. If, farther, we compare the results of the

two series of experiments made by Messrs Allen and Pepys, the difficulties, in a physiological point of view, greatly accumulate upon us. For, if an *absorption* of oxygen really take place in the lungs, how does it happen, that, in the first thirteen experiments, made with several 1000 cubic inches of air, and which occupied from ten to twenty-four minutes of time, a very small loss in the whole bulk of air, and not the smallest in its proportion of oxygen, occurred ; while, in two other experiments, made with only 300 inches of air, and continued only for three minutes of time, a great deficiency in the whole bulk of air, and a loss of one-third of its oxygen, took place. In all these experiments, except the twelfth, in which, instead of loss, there was actually an increase of eleven cubic inches upon the bulk of air respired *, the same person appears to have breathed, and the air was of similar composition. Consequently, the cause of variation in the result is to be sought, not in any difference in the animal organs, or in the original composition of the air, but, probably, in some circumstances of dissimilarity, which accompanied the progress of the experiment.

575. Now the bare statement of facts points out a great dissimilarity, not only in the chemical results, but in the circumstances accompanying the experiments, and in the effects which they produced in the system. For in the first thirteen experiments, which occupied from ten to twenty-four minutes, and in which no loss

* Phil. Trans. 1808, p. 256.

of oxygen occurred, the air was only *once* passed through the lungs, the breathing was nearly natural, the operator scarcely fatigued, and his pulse not raised more than about one beat in a minute*. But in the two experiments, in which oxygen is said to be absorbed, the same air was passed *eight or ten times* through the lungs; and, in less than a minute, the operator found himself obliged to take deeper and deeper inspirations. At last, the efforts to take in air became very strong and sudden, with a great sense of oppression and suffocation in the chest, indistinct vision, buzz in the ears, loss of recollection, and, at the end of three minutes, perfect insensibility†. This difference in the effects produced in the system, we do not hesitate to ascribe to a difference in the *composition* of the air (91. 92.), which, in the first experiments, was respired in a natural state, but, in the two last, by repeated breathing, was rendered more and more unfit to carry on respiration, until, at length, its power of supporting that function altogether ceased.

576. But because under circumstances, in which the mental and animal powers were in complete abeyance, the respiratory organs were not able to make so complete an expulsion of the inspired air as they effect in their natural state of health and vigour, are we, therefore, entitled at once to conclude, that all the air which was not expelled was really *absorbed*? Setting aside the anatomical difficulties in the case, let us, for a moment, look only to the chemical conse-

* Phil. Trans. 1808, p. 253. † Ibid. p. 260. 262.

quences, to which such a conclusion would conduct us. If the mere disappearance of any gas, received into the lungs, be sufficient evidence of its absorption, then every gas, which is not returned, must be held to be absorbed. Are we then prepared to admit that hydrogen and nitrogen gases are absorbed by the blood? for, when their respiration is carried to its full extent, they too equally disappear. This supposed absorption, however, cannot proceed from the operation of chemical attraction, for little or no affinity (98. 104.) subsists between these gases and the blood. Neither can it arise from the operation of the living system; for it occurs only when the living powers are about to cease. To us there appears but one way of escaping from these manifold difficulties, which is simply to conclude, that the inspired air, which is not returned, is retained in the cells of the lungs. Such a supposition dissipates at once all anatomical and chemical difficulties, and explains why no air disappears in natural respiration, when the expiratory powers are in full vigour and able to expel it, and why its disappearance increases in proportion as the actions of these powers decline and cease.

577. It is, however, worthy of remark, that, in these last experiments, not only was there a diminution in the whole bulk of air, but its relative proportions likewise varied; for, in 100 parts, the oxygen and carbonic acid amounted together only to about two-thirds of the usual quantity of oxygen, and the deficiency was supplied by a superabundance of nitrogen gas. We are not prepared to say why, in this very embarrassed state of the respiratory function,

the relative proportions of the expired air should thus vary ; but the fact proves only the retention of oxygen in the lungs, but not its absorption by the blood. Should it even be maintained that oxygen was absorbed, because, in these two experiments, a portion of it disappeared, then, by the same mode of reasoning, we must also contend, that, in the thirteen preceding experiments, no absorption of oxygen took place, because no part of it was retained ; and as these last experiments alone come near to the natural exercise of this function, they authorise us to conclude, that such supposed absorption of oxygen constitutes no necessary part of healthy respiration. In truth, in some instances where a mixture of oxygen and hydrogen gases was respired, the oxygen and carbonic acid in the expired air uniformly exceeded, by one *per cent.* the total oxygen inspired * ; from which it may be inferred, that these variations in the proportions of the expired air proceed entirely from accidental causes, and are totally independent of any absorbent function in the lungs.

578. But, beside the argument derived from the supposed loss of oxygen in respiration, analogies and experiments of a different nature have been brought forward in support of the doctrine that oxygen combines with the blood. It is known that the blood becomes red, when it is placed in contact with oxy-

* Phil. Trans. 1809, p. 425.

gen gas ; and may not the base of this gas produce this effect by combining with the blood ? Mercury, lead, and iron, says M. Lavoisier, become red when they combine with oxygen, and since the blood is rendered red by pure air, may not its redness proceed from a similar combination * ? By very decisive experiments, M. Lavoisier shewed, that the base of oxygen gas combined with metals, during their conversion to the state of oxides ; but we have suggested doubts (427.), whether the red colour, even of metallic oxides, depends on any colouring property in oxygen ; and were this, for the present, conceded, it would not serve the case before us, since we possess no direct evidence that oxygen combines with the blood. It is, therefore, incumbent upon those who believe the blood to be rendered red in consequence of its oxygenation, to afford some palpable evidence that the base of oxygen gas really combines with that fluid, in a manner similar to that in which it unites with metals.

579. It does, indeed, appear to us somewhat remarkable, that M. Lavoisier, whose accurate and comprehensive views respecting the combinations of oxygen, enabled him to introduce such great improvements into the general doctrines of chemistry, and who first clearly remarked (118.) the difference between the effects produced in the air by combustion and by respiration, should still, apparently from the mere circumstance of similarity of colour, have concluded that respiration was nothing but a simple

* Mem. de l'Acad. des Sciences, an. 1777, p. 192.

combustion, and its result an oxidation of the blood. To establish his theory of oxidation, M. Lavoisier trusted not to colour alone, but actually proved the combination and disengagement of oxygen, as the metal was alternately reduced or revived. In the process of oxidation, he remarked that the air lost one-sixth of its bulk, and contained no carbonic acid; in that of respiration, the loss of bulk was only $\frac{1}{80}$, and a quantity of carbonic acid was produced. He even found that caustic alkali abstracted one-sixth of its bulk from air that had been respired, and therefore concluded that such air contained one-sixth of carbonic acid; and after this acid gas was abstracted, the residue exactly resembled that left by the calcination of metals *.

580. Thus, then, it appears, that although, in these experiments, M. Lavoisier distinctly ascertained, that, in oxidation, all the oxygen combined with the metal, and no carbonic acid was formed; and, on the contrary, that, in respiration, all the oxygen was converted into carbonic acid, and none therefore could combine with the blood; yet he, nevertheless, supposed this oxygen to unite with the blood, and to produce effects in it similar to those which followed the actual combination of this element with metallic matter. M. Lavoisier and his associates exclaimed loudly against their predecessors for employing phlogiston as a principle, which, like another Proteus, was able to exhibit every colour, and to assume every shape *; but we may safely assert, that there is scarcely any

* Mem. de l'Acad. 1777, p. 185. † Ib. an. 1789, p. 568.

colour or shape in nature, which, in their turn, these philosophers have not made oxygen to exhibit and assume. And if it was absurd in the Phlogistians to ascribe effects to an agent which they could not prove any where to exist, it is, surely, not less absurd in their successors to attribute similar effects to oxygen, even where it exists not.

581. It is, farther, an objection to this supposed operation of oxygen, that, in the lungs, the blood and air do not come into contact, and, therefore, although the combination of oxygen with that fluid might be conceived to happen when they are placed together out of the body, yet the intervention of organised membranes may be supposed to prevent such an union in the living system. In the ordinary operations of chemistry, such an interposition of animal substance would be considered sufficient to vitiate the result of any similar experiment in which it was employed; but in the application of this science to the living body, neither membranes nor blood-vessels are conceived to oppose any obstacle to the exertion of chemical action, or, in the smallest degree, to affect its result. In support of this supposed operation of oxygen on the blood, some experiments of Dr Priestley have been appealed to, as affording decisive evidence that this substance has the power of penetrating a compact membranous body, and may, consequently, penetrate the cells and blood-vessels of the lungs. The importance which has been attached to these experiments, in all the late hypotheses which have been proposed to explain the function of respiration, renders it necessary for us to examine them

with some minuteness, in order to discover the true relation which they bear to the present question.

582. Dr Priestley, who, as we shall hereafter see, supposed that venal blood became red by imparting its phlogiston to the air, knew well that the blood in the lungs was separated from the air by a membranous substance, which, however, according to Dr Hales, does not, in thickness, exceed the $\frac{1}{1000}$ part of an inch. To ascertain the effect of this circumstance, he put some black blood into a bladder moistened with a little serum, and then tying the bladder very close, he hung it in a free exposure to the air. The next day, all the lower surface of the blood, which had been separated from the air by the intervention of the bladder, had acquired a coating of a florid red colour, as thick, it appeared, as it would have acquired, if it had been immediately exposed to the open air; so that this membrane had been no impediment to the action of the air on the blood. This experiment was repeated, without previously moistening the bladder, and with the very same result *.

583. But although, in these experiments, the blood was rendered red by the agency of the air, yet we are not entitled to conclude, that this redness was produced by the combination of its oxygen, unless we can shew not only that this substance comes into contact with the blood, but is likewise capable of changing it to a red colour. Dr Priestley himself, who believed the blood to become red by the loss of phlogiston, could draw

* Obs. on Air, abridged, vol. iii. p. 369.

no such conclusion ; and it is not a little remarkable that this philosopher, who had before so well observed the reciprocal effects produced in the air, when it thus changed the colour of the blood *, should, in these experiments, have entirely overlooked them. It is still more remarkable, since these experiments have drawn so much attention, and seem now to be the chief or only remaining evidence urged in support of the hypothesis of oxygenation, that some attempt has not been made to inquire farther into the actual circumstances which attend them. It is this examination which we now propose to make, in the hope, that, if it do not lead us to a knowledge of the true cause of this phenomenon, it may at least serve to shew to what it is not to be ascribed.

584. With this view, we procured a quantity of black blood, and putting it into a sheep's bladder, suspended it from the top of a jar containing about 100 cubic inches of atmospheric air. The jar was inverted in a saucer containing mercury, and within it a small cup of solution of potassa was likewise placed. The blood, in a short time, assumed a florid hue, and a dimness extended over the inside of the jar. By the next day, the mercury in the saucer had risen $\frac{2}{10}$ of an inch into the jar, and it continued to rise several days ; so that, by the fifth day, it had reached nearly to an inch in height. The jar was then raised, and diluted acid being poured upon the alkaline solution, disengaged from it a large quantity of carbonic acid gas. By this experiment, there-

* Obs, on Air, vol. iii, p. 336.

fore, we are taught, that, when black blood assumes a red colour by being thus placed in a moistened bladder, and exposed to atmospheric air, the air itself, at the same time, undergoes a change; for its volume is diminished, and carbonic acid is produced.

585. To ascertain these facts with greater precision, we put another quantity of black blood into a small bladder, and suspended it, as before, from the top of a small jar inverted in mercury, and which contained 18.3 cubic inches of atmospheric air. Under this jar, also, a small cup of solution of potassa was placed. The blood, as before, was soon reddened, and the jar became dim. In two days, the mercury had risen nearly half an inch into the jar, and, by the close of the fourth day, it stood seven-eighths of an inch high, where it remained for some time quite stationary. On analysing the residual air, it was found to suffer no change, either from agitation with lime water, or by being exposed to the contact of phosphorus; so that, though all the oxygen had disappeared, no carbonic acid was present, but that gas was entirely attracted by the water of potassa employed.

586. The capacity of the jar, in the above experiment, has been stated to be equal to 18.3 cubic inches; and the bladder, with its contents, together with the cup and solution, we found to occupy a space equal to 5.2, which reduces the actual bulk of air, employed in the experiment, to 13.1 cubic inches. The mercury which, in consequence of the attraction of the carbonic acid, had risen seven-eighths of an inch into the jar, occupied a space equal to

three cubic inches ; so that, of the 13.1 inches of air originally employed, three had disappeared, and $\frac{10}{13.1}$ or a portion of the air was thus converted into carbonic acid, which comes very near to the proportion of oxygen gas which the atmosphere is known to contain. Hence we infer, that, in this experiment, all the oxygen gas that disappeared was converted into carbonic acid ; and, consequently, we deny that any oxygen penetrated the bladder, in order to combine with the blood.

587. As thus it is denied that the blood, in these experiments, received any ponderable matter from the air, so likewise it will appear, from the facts which follow, that the air receives no such matter from the blood. We filled bladders with water, and suspended them in jars of atmospheric air, in the manner described above ; and found that the oxygen gas of this air was converted into carbonic acid, in the same manner as when the bladders were filled with blood ; and if the experiment was continued a sufficient length of time, the whole of the oxygen gas was, in like manner, made to disappear. The same effects followed from the introduction of moistened empty bladders ; and, indeed, it is the usual effect produced in the air by every moistened animal substance. If, therefore, the moistened bladder be thus capable, by itself, of acting on the air, we are entitled to conclude that it exerts the same action when it is filled with blood ; and as, on this supposition, the oxygen gas will unite with the carbon, furnished directly by the bladder, we have no ground whatever to suppose this carbon to come from

the blood. Hence, therefore, when black blood is reddened by the air, through the coats of a moistened bladder, the air yields no oxygen to the blood, nor acquires from it any carbon; but the carbon of the bladder, by its combination with the oxygen of the air, passes into the state of carbonic acid gas.

588. Even Dr Priestley, who neglected to observe the effects produced in atmospheric air, by the contact of bladders, was well aware of the action they exerted on nitrous gas, which we now know to be composed of the same elements as common air. He observed an incrustation of lime to appear, when nitrous gas, which had been previously kept in a bladder, was mixed with lime water; or, when this gas was transferred from one vessel to another through a bladder, it produced a similar effect. To prove that this effect arose from the bladder, he put a bladder into a jar of nitrous gas, and, after it had continued there twenty-four hours, he transferred the gas into a glass-vessel of lime-water, which it rendered turbid *. In other experiments, when nitrous gas was left in moistened bladders, it completely lost its property of diminishing common air; but if the bladder was kept quite dry, the gas then underwent little or no change †. He even found that nitrous gas was, in like manner, deprived of its oxygen, by being placed in contact with blood, by which it was rendered unfit for eudiometrical purposes ‡.

589. The remarks, which have now been made,

* Obs. on Air, vol. i. p. 213.

† Obs. on Air abridged, vol. iii, p. 389. ‡ Ibid. p. 367.

on the mode in which moistened bladders act upon the air, may be extended to the action of all other animal membranes. In an experiment made by Mr Hunter, black blood was reddened by the air, when covered by goldbeater's leaf, which touched its surface *. But this leaf is an animal substance, and when, therefore, it was moistened by coming into contact with the blood, it acted on the air, like the bladder mentioned above, and to this action the red colour of that fluid succeeded. Dr Goodwyn, also, laid bare the jugular veins of rabbits, and, having intercepted the blood by ligatures, directed upon the coats of the veins a stream of oxygen gas. The blood, in some instances, became a little florid; but, in other cases, no change of colour ensued, although the stream of oxygen was kept up for two minutes. As, however, the change occurred in some instances, *something*, says Dr Goodwyn, must have pervaded the vessels, and this, probably, by the force of chemical attraction; but what that is, he adds, is not yet known, whether it be some principle escaping from the blood to form fixed air, or whether it be a part of the oxygen itself that enters into the blood. It is certain, however, that the change of colour which the blood undergoes, is occasioned by the chemical action of the air †.

590. In these experiments of Dr Goodwyn, there can be little doubt but that the oxygen gas of the air was more or less changed into carbonic acid;

* Treatise on the Blood, p. 62.

† Connex. of Life with Respiration, p. 63.

and the variation in appearance, which the blood exhibited, would correspond with the degree in which that change took place. An exhalation of fluid is constantly going on from all the moistened surfaces of the body, and may easily be conceived to have issued in a greater or less degree, from the recently exposed veins of the rabbits. According, therefore, to the activity with which this process proceeded, the colour of the blood would be more or less rapidly changed. In the same manner, Dr Barclay observes, that the blood in the lungs, when exposed to the air through the medium of its vessels, is always observed to change its colour a great deal faster, when the exhalents continue to act with a vital energy, than when they act slowly and feebly, as inanimate organs *; and we have elsewhere remarked (143.), that, when the exhalent function has entirely ceased, no change is induced on the air, and no apparent change seems then to be effected in the blood.

591. It is not, however, necessary that the carbonic matter, which combines with oxygen, should be afforded by a living action, else carbonic acid could not be formed by dead moistened bladders, nor by the blood after its removal from the body. In substances deprived of life, therefore, the carbon may rather be said to escape by evaporation, than by exhalation, which, physiologically speaking, is a living action; but in whichever way this matter come into contact with oxygen gas, carbonic acid is form-

* On Muscular Motion, p. 524.

ed, and the consequent effects on the blood, whether it be present in the body, or be withdrawn from it, will take place. In every case, however, a due degree of moisture and heat must be present to favour this chemical union; hence dry bladders produce no change in the air, and it remains equally unchanged by moistened bladders, if the necessary degree of heat be withdrawn.

592. That this change in the colour of blood is always accompanied by a corresponding change in the air, may be farther inferred from other experiments of Dr Priestley. He found, that, when the black crassamentum of blood was covered by serum or milk, it nevertheless acquired a florid hue, on being exposed to the air*; and Dr Wells observed, that a covering of albumen, also, did not prevent the action of the air on the blood†. Now we have already seen, that serum (97.) and albumen (153.) convert the oxygen gas of the air into carbonic acid; and we found by experiment, that the same effect was produced by milk, as probably would be the case with most of the animal fluids. Hence, it is evident, that, when the blood, in the experiments of Dr Priestley, became florid, through several inches of serum, the oxygen gas must have been at once changed by it into carbonic acid, and could never therefore, in the form of oxygen, be conveyed through this fluid to act on the blood.

593. On the other hand, Dr Priestley found a thin

* Obs. on Air abridged, vol. iii. p. 370.

† Phil. Trans. 1797.

stratum of water to prevent entirely this action of air on the blood *. M. Cigna found the same thing to take place, when a pellicle of oil was interposed † ; and Dr Wells ascribes a similar effect to a solution of gum arabic. These substances, however, act little, if at all, in changing the air ; and no change of colour, therefore, takes place in the blood. That black blood should have the power of attracting the oxygen of the air, through several inches of serum, and yet lose this power when a thin stratum of water is interposed, seems somewhat surprising, if the intervening fluid be, in each case, considered to be equally passive ; but proceeding on the fact, that the serum exerts an action on the air, which the water is incapable of effecting, a new circumstance comes into view, and upon it the colouration of the blood may probably depend.

594. If, then, it appear, that the interposition of substances between the blood and the air necessarily prevents that contact which is essential to the chemical union of oxygen with that fluid ; if it also appear, that the colour of the blood is never, in such cases, changed, unless such substances be interposed, as are themselves capable of acting on the air ; and if, lastly, it be proved, that when the blood exhibits this change of colour, the air also suffers change, and that its oxygen, instead of combining with the blood, is really contained in the carbonic acid that is formed, we must conclude, that, whatever be the

* Obs. on Air, abridged, vol. iii. p. 370.

† Ibid.

mode in which the air contributes to change the colour of the blood, it cannot be by imparting to it any portion of its ponderable matter. Consequently, although these facts prove that oxygen gas possesses the power of changing the colour of the blood, as well through dead, as through living, animal membranes, yet they afford no evidence of the combination of oxygen with that fluid, but shew only the conversion of that gas into carbonic acid, precisely in the same manner as this acid gas is formed when the blood is reddened in the ordinary process of respiration.

595. Even when the air and blood are brought into contact, they only exert a reciprocal action on each other, by which carbonic acid is formed, but no oxygen appears to combine with the blood. We have already given various proofs (97.), that, when the blood is changed in colour by the agency of the air, the oxygen gas of the air disappears, and carbonic acid is produced. These facts are confirmed by the experiments of M. Berthollet, who confined recent blood in a vessel of common air, and, at the end of twenty-four hours, the air, on analysis, afforded nearly $\frac{4}{100}$ of carbonic acid. In two other experiments, similar results were afforded; and in all these experiments, the acid gas produced was exactly equal to the volume of oxygen that disappeared*. Unless, therefore, it be maintained, that the same oxygen can, at the same time, exist in two combinations, we must suppose, that, in these experiments, no oxygen com-

* Mem. d'Arcueil, t. ii. p. 462.

bines with the blood; and from whatever cause, therefore, the red colour of the blood may proceed, we may safely conclude, that it cannot arise from the combination of oxygen. It forms no part of our present intention to inquire farther into the cause of this redness of the blood; it is sufficient, for our present purpose, to have shewn that it cannot proceed from its oxygenation.

596. In our former work, we maintained (129.), not only that no oxygen entered the blood, but that the same circumstance might be affirmed of the nitrogenous portion of the air. This we stated to have been uniformly the opinion of M. Lavoisier, who, we are told, ascertained, by rigorous experiments, that neither an increase nor diminution of nitrogen occurred in respiration *. Such too was the conclusion of Goodwyn and Menzies; and it is borne out by the results afforded in the respiration of all the lower animals. In his late experiments, Dr Thomson observed a deficiency in the quantity of nitrogen; but it was very inconstant, and sometimes scarcely perceptible; and he therefore deems it rather an accidental, than necessary, condition of respiration †. In the experiments of Messrs Allen and Pepys on the respiration of a Guinea pig (568.), no change whatever occurred, either in the quantity, or the quality of

* Mem de l'Acad. an. 1789, p. 574.

† Syst. Chem., vol. v. p. 737, 3d edit.

the nitrogen gas; and in their experiments on human respiration (571.), the deficiency in bulk was so extremely small as to preclude all idea that nitrogen is necessarily retained in the system. Notwithstanding, also, this small deficiency in the whole bulk of air, yet its constituent parts, (estimating the carbonic acid as oxygen) maintained their usual proportions, the nitrogen gas amounting always to $\frac{79}{100}$, which could hardly have happened, if any portion of that gas had been abstracted from the general mass, and retained permanently in the system. In the late experiments, also, of M. Berthollet, no deficiency, but rather an excess of nitrogen appeared in the air expelled from the lungs *. Lastly, nitrogen gas is not absolutely necessary to the exercise of this function; for we have seen (127.) that animals live very well in pure oxygen; and both M. Lavoisier, and Messrs Allen and Pepys, found, that hydrogen gas might, also, be made to supply its place. These facts, therefore, entitle us to repeat, that nitrogen gas exerts no direct agency, nor suffers any apparent change, during the exercise of the respiratory function.

597. We, however, before remarked (107.), that Drs Priestley and Henderson, Mr Davy and others, from finding a smaller volume of nitrogen to be, in some cases, expelled, than had been previously taken into the lungs, concluded that a portion of this gas was *absorbed* in respiration. This opinion we contested, both on anatomical and chemical grounds, and endeavoured to shew (92.), that the error arose

* Mem. d'Arcueil, tom. ii. p. 459.

from the embarrassment into which the respiratory organs were brought by the respiration of an impure air, which rendered them incapable of effecting so completely, as usual, the expulsion of the air they received, and consequently led to its more abundant retention in the lungs.

598. As it was then our fortune to contend against what was called an *absorption* of nitrogen by the blood, so now we have to combat what has been named an *evolution* of this gas from that fluid. In most of the experiments related by Messrs Allen and Pepys, when the same person was employed to breathe, only a small deficiency occurred in the whole bulk of air ; but, in one experiment, in which a different person operated, there was an increase of eleven cubic inches upon the whole bulk of air employed. As, however, this air contained the usual proportions of oxygen and nitrogen, no particular effect could have been exerted upon either of its constituent parts ; and the “ excess, therefore, of 11 cubic inches,” to use the expression of these able chemists, “ no doubt arose from the person not having been in the habit of exhausting his lungs, so that they contained more when he began, than when he left off *.”

599. But in some experiments on the respiration of oxygen gas, which contained only 2.5 *per cent* nitrogen, Messrs Allen and Pepys were enabled to obtain nearly all the residual gas from the lungs, by

* Phil. Trans. 1808, p. 256.

furnishing oxygen gas to supply its place. After having made a forced expiration, the operator began to respire oxygen, until nearly all the air, previously existing in the lungs, was expelled. This air, partly by experiment, and partly by calculation, was found, at temperature 53° , to amount to 141 cubic inches, which consisted of 22.56 oxygen and carbonic acid, and 118.54 nitrogen. Hence, it is inferred, that, after a forced expiration, the lungs of this person must have contained 141 cubic inches of air, which, at temperature 97° , would be increased in bulk to 154 cubic inches *. In another experiment of the same kind, the volume of air in the lungs is estimated at 226 cubic inches, of which the oxygen and carbonic acid formed together about one-fifth †.

600. The large portion of nitrogen gas, which thus appeared to exist in the lungs after the most forcible attempt at expiration, induced these chemists to repeat their experiments on the respiration of oxygen gas. The experiment was made with 2668 cubic inches of oxygen, which contained four *per cent.* nitrogen. The gas was breathed for 13 minutes, and was afterwards analysed. The total nitrogen in the gas inspired was 106.72; in that expired, 211.80; so that 105.08 cubic inches of nitrogen had been obtained from the residual air of the lungs. “The question, therefore, is, whether this increase of nitrogen can be owing to the residual gas contained in the

* Phil. Trans. 1808, p. 266.

† Ibid. p. 276.

lungs at the beginning of the experiment, or whether a portion of oxygen is not actually exchanged for nitrogen, when pure oxygen is respired *."

601. The determination of this question, it is justly observed, will be much influenced by the actual capacity of the chest, and the quantity of air which it is able to contain. We have already given various estimates of this kind, from which we inferred (89.), that, after a natural expiration, or the final expiration that is made at death, the lungs contain from 109 to 118 cubic inches of air; that, after a forced expiration of air during life, or after that state which succeeds to drowning, the quantity of air is much smaller; while, in violent death from hanging, it sometimes amounts (90.) to 250 or 270 cubic inches. From a particular method, employed by Messrs Allen and Pepys, they make the volume of air in the lungs, after complete expiration at death, to occupy about 108 cubic inches; but the mean of their experiments, on the respiration of oxygen, would make it equal to 183: we are, therefore, say they, "almost compelled to allow, that, when pure oxygen is respired, a portion of nitrogen is given off by the blood †."

602. In subsequent experiments of a similar nature on a Guinea pig, that animal was made to breathe, for one hour and twelve minutes, without apparent inconvenience, 1060 cubic inches of oxygen gas. The gas, before respiration, contained 97.40 inches

* Phil. Trans. 1809, p. 409.

† Ibid. p. 412.

nitrogen ; and, after the experiment, it contained 147.52 ; so that 50.12 cubic inches of nitrogen had been obtained from the animal. This increase of nitrogen, it is added, was much more than equal to the cubic contents of the animal's body *. In another experiment, the excess of nitrogen was 34.20 ; and in a third, made with a mixture of oxygen and hydrogen gases, the nitrogen obtained from the lungs of a Guinea pig amounted to 57.40 cubic inches †. These experiments seem clearly to shew, that, under certain circumstances, a volume of nitrogen gas is obtained from the lungs, greater than the actual capacity of those organs, and greater, consequently, than, in conformity with the mechanical properties of elastic fluids, they can be considered able to contain.

603. Is then this excess of nitrogen, as these chemists suppose, really given off from the blood ? This question necessarily involves the previous considerations,—Does this gas, at any time, exist in that fluid, and from what sources can it be derived ? Now we have no evidence that nitrogen gas is contained in the blood, but the base of that gas is known to exist in it ; and, by chemical agents, which effect the entire decomposition of the blood, it may be obtained from it in a gaseous form. It can hardly, however, be contended, that, because the blood affords nitrogen gas under decomposition, it yields it spontaneously in the healthy living system, where no such decomposition takes place ; though this, indeed, is the

* Phil. Trans. 1809, p. 417. † Ibid.

sort of analogical evidence, on which the greater part of our chemical physiology has been made to rest. Until, however, some direct evidence be adduced, that healthy blood undergoes such changes as will enable it to yield its nitrogen in a gaseous form, we may venture to deny, that the nitrogen gas, afforded in these experiments, was derived immediately from any natural changes occurring in that fluid.

604. Rejecting, therefore, the supposition, that the large quantity of nitrogen gas, obtained in these experiments, is derived from any spontaneous changes or decompositions going on in the blood itself, we are necessarily compelled to consider it as afforded by the residual air contained in the lungs. In support of this opinion, it may be stated, that the production of nitrogen is always greatest in the first expiration, and that its quantity progressively diminishes, until, towards the close of the experiment, it is reduced almost to nothing * ;—circumstances which seem plainly to shew that nitrogen is no longer obtained, when all the residual air, previously existing in the lungs, is removed. If this nitrogen were furnished by the blood, independently of the residual air, no reason occurs why it should thus diminish and cease to appear, as this air is abstracted ; for the function of respiration goes on, and the blood, as far as depends on itself, cannot be considered less fit to supply nitrogen. The fact, also, that no such excess of nitrogen is furnished in natural respiration,

* Phil. Trans. 1809, p. 409.

militates against the notion of its proceeding directly from the blood.

605. Assuming, then, that the nitrogen in question is primarily derived from the air contained in the lungs, can we suppose this gas first to enter the blood, and to be subsequently expelled from it? for such operations must take place if nitrogen be really given off from the blood. Now air can be conceived to enter the blood only by the force of mechanical pressure, by absorption, or by the exertion of chemical affinity. If it entered by the first mode, the structure of the cells and vessels would, probably, be irrecoverably injured, and the presence of the air in the vessels would speedily destroy (103.) life; if by the second mode, the air gained admission, it would be carried, by the absorbents, into the venal circulation (95.), which few will suppose to happen; and, lastly, it cannot be considered to enter by the exertion of chemical attraction, not only because the cells and vessels are interposed between the blood and air, but because no affinity subsists (110.) between nitrogen gas and blood. Denying, therefore, the possibility of nitrogen gaining admission into the blood vessels by any mode that has been hitherto suggested, and knowing no way in which this gas can be supplied by that fluid, without supposing it previously to enter into it, we are necessarily obliged to deny also the *evolution* of nitrogen from the blood.

606. We think, too, that the circumstances required to accomplish this supposed *evolution* of nitrogen, oppose the belief that it ever takes place; for it not only does not occur in the ordinary exercise of

this function, but no excess of nitrogen is ever afforded in other cases, unless its place be supplied by an equal or superior bulk of some other gas. Thus, when nearly pure oxygen was breathed, a bulk of that gas disappeared, equal to that of the nitrogen obtained; and when a mixture of oxygen and hydrogen gases, in the proportion of $\frac{12}{100}$ of the former to $\frac{18}{100}$ of the latter, was respired, then, also, a bulk of hydrogen disappeared, equal to that of the nitrogen obtained; so that when nearly pure oxygen gas is respired, say Messrs Allen and Pepys, a portion of it is missing at the end of the experiment, and its place is supplied by a corresponding quantity of nitrogen; and the same thing, they add, takes place when an animal is made to breathe a mixture of hydrogen and oxygen gases*.

607. These facts appear to afford evidence, that, in this supposed evolution of nitrogen from the blood, nothing more than a mechanical substitution of one gas for another takes place; and as in these experiments oxygen and hydrogen caused the expulsion of nitrogen by occupying its place, so we have no doubt but that, when atmospheric air was again breathed, these gases were in turn expelled, and the lungs recovered their former proportion of nitrogen gas. This must, we think, of necessity happen; for we have seen that oxygen suffers no loss of bulk in the lungs (567.), but is only changed into carbonic acid; and Mr Davy long since proved (104.), that hydrogen gas, in its respiration, undergoes no apparent altera-

* Phil. Trans. 1809, p. 427.

tion. Hence, then, we conclude, that the nitrogen gas, obtained in these experiments, is derived from the residual air existing in the cells of the lungs, and that its displacement is occasioned by the mechanical substitution of another gas.

608. Abiding, however, by this conclusion, and yielding, as we are disposed to do, assent to the accuracy of the results which these experiments indicate, we lay ourselves open to difficulties of another kind; for on comparing the state and capacity of the lungs, both in man and in the Guinea pig, with the actual volume of air which they respectively afford, how, it may be asked, are we able to account for the retention of a quantity of air in the lungs, apparently superior in volume to the capacity of the containing organs? Even at the ordinary state of temperature and density in which the air enters the lungs, this difficulty presents itself; and exposed, as this air afterwards is, to so great an increase of temperature, its volume, in conformity with the mechanical constitution of gaseous bodies, will be proportionally augmented. To us there occurs no other mode of relieving ourselves from this difficulty than by supposing that the air, instead of expansion, really suffers a reduction of its elasticity, in the lungs.

609. We have seen (114.), that the air, on entering the lungs, is diffused through an infinite number of small cells, whose diameters do not exceed the $\frac{1}{16}$ part of an inch; and if these cells, as some have supposed, possess a contractile power, either derived from their own structure, or from the action of the thick vascular plexus that is distributed upon them,

they may, although individually their action be small, produce nevertheless, by their combined operation, an effect of considerable magnitude and power. We confess, however, that another circumstance, arising out of this peculiar structure, offers, in our opinion, a better explanation of this supposed fact. It is well known, that a strong attraction or *adhesion* is exerted between air and the surfaces of all bodies, so that from many bodies it is detached with considerable difficulty. The more, therefore, that we increase the extent of surface between the air and the bodies with which it is brought into contact, the more do we augment the effect of this attractive force. Now the surface of the cells of the human lungs, independently of that of the bronchi, is estimated by Dr Keill at 21907 square inches (114.), which is ten times greater than that of the whole body. If, therefore, we call to mind, that no part of the air in these cells can be at a greater distance from their surface than half their diameter, or the $\frac{1}{16}$ part of an inch, and consider, also, the great extent over which this attraction operates, we offer no apparent violence to probability in believing that such a force, acting to such an extent, may constrain the elasticity of the air, and considerably reduce its volume.

610. That moistened animal substances exert a strong attraction towards gaseous fluids, may be inferred from certain facts which occur in the respiration of fishes. There can be little doubt, but that the oxygen gas, which is converted into carbonic acid by the gills and skins of these animals (550.), is attracted by the animal substance, and

brought into such state of contact, as to favour its union with the animal carbon. When, also, water is exposed to heat, its elastic fluids are in great part expelled, but the last portions of oxygen adhere with so much force, that they cannot be separated even by long continued ebullition; yet M. M. Humboldt and Provençal found, that, in such water, fishes were able to respire, and to convert its remaining oxygen into carbonic gas, although the oxygen did not exceed $\frac{1}{3000}$ of the volume of water employed *. While this fact, therefore, points out the admirable provision that is made, to secure to fishes, even under the most unfavourable circumstances, the due exercise of this necessary function, it indicates, at the same time, the existence of a strong attractive force between the animal substance and oxygen gas. So, likewise, in many experiments made by Spallanzani, the volumes both of oxygen and nitrogen gases, placed in contact with animal substances, were, for a time, considerably diminished; which must have been accomplished by some property in the animal substance, independent of its living power. In the same manner, we have seen, that the cellular and fleshy leaves of certain plants are able to receive and contain a volume of air superior to their own bulk (342.); but, perhaps, the chemical affinity, exerted between this air, after its conversion into carbonic acid, and the saline ingredients of the plant, may be deemed sufficient to account for the phenomenon. In the experiments, however, of Mr Acton (217.), dead vegetable matter

* Mem. d'Arcueil, tom. ii. p. 381.2.

was found to reduce the volume of oxygen gas, as well as living matter, in which case no saline substances could have been materially concerned.

611. In the example of charcoal, also, we possess facts which bear a near analogy to the circumstances under consideration. Scheele, Priestley, and Fontana remarked, that charcoal, after having been heated, possessed the singular property of attracting and condensing air; and that neither the air nor the charcoal suffered, in consequence, any change in their chemical properties. These experiments were repeated by Morozzo, who heated pieces of charcoal of a determinate size, and then passed them, in an ignited state, into tubes of mercury, where they remained till they became cold. Different gases were then passed up into the tubes, and were attracted and condensed in very different proportions. Atmospheric air was condensed in a proportion three times greater than the bulk of charcoal employed; hydrogen and oxygen in a bulk nearly double that of the charcoal; and carbonic and muriatic acid gases, ammonia, and sulphuretted hydrogen, in a bulk about eleven times as great. These experiments were repeated by Rouppe and Van Noorden, who employed an apparatus, in which the ignited charcoal was allowed to cool, without being plunged in mercury, or exposed to the contact of air. Atmospheric air and oxygen were then condensed by it in a volume about three times greater than the piece of charcoal employed; nitrogen and hydrogen gases in a somewhat smaller proportion; nitric oxide gas in a bulk more than eight times as great; and carbonic acid

gas in a volume nearly fourteen times greater than that of the charcoal. Morozzo again resumed his experiments, with the precautions employed by Rouppe, and, upon the whole, obtained results nearly similar to those of his former trials. During this remarkable attraction and condensation, the elastic fluids appear to suffer no change in their properties, but are again expelled unaltered by low degrees of heat; neither does the charcoal itself suffer any change, except from a small increase of weight. This attraction of gases by charcoal, says Mr Murray, can scarcely be denominated chemical affinity, but the union between the gas and charcoal must be regarded as rather of a mechanical nature. Gases, indeed, are thus condensed, to which, under any known circumstances, charcoal does not exert any chemical attraction; and they are all reproduced pure by a slight elevation of temperature *.

612. The foregoing experiments, thus repeated and varied by so many different philosophers, and which, as to their general results, are not easily exposed to deception, are nevertheless pronounced "vague and contradictory" by Mr Dalton, who seems inclined entirely to discredit them, and that too upon the faith of the following experiment. He made 1500 grains of charcoal red hot, then pulverized it, and put it into a florence flask, with a stop-cock; to this flask, a bladder, filled with carbonic acid, was connected, and the gas was then admitted into the flask, which became a little heavier. During the ex-

* Syst. Chem. vol. ii. p. 317. et seq.

periment, the carbonic gas entirely disappeared, but what became of it Mr Dalton does not state; he denies, however, its attraction by the charcoal, and asserts that the increase of weight, which that substance received, proceeded entirely from the moisture of the bladder. He considers the attraction of moisture to be the only cause of the increase of weight, which newly-made charcoal receives; and to the decomposition of this moisture, and the union of its elements with charcoal, he ascribes the abundance of gases obtained from charcoal at a red heat. Mr Dalton adds, that the expulsion of gases from charcoal by the heat of boiling water, as alleged in these experiments, is certainly not true, as Allen and Pepys have shewn; and most practical chemists know, says he, that no air is to be obtained from moist charcoal below a red heat*.

613. Notwithstanding the confidence with which Mr Dalton delivers himself on this subject, we shall proceed to point out a few circumstances in his experiment, which prevent our acquiescence in his conclusions. As he seems to have relied chiefly on the circumstance of *weight*, for determining the alleged attraction of these gases, it might have been expected, that, like his predecessors, he would have employed an apparatus which could not have communicated moisture to the charcoal, instead of the bladder which he actually used, and which, he says, did communicate moisture. But the circumstance of increased weight does not afford such satisfactory

* Chem. Phil. part ii. p. 235.

evidence of this alleged fact, as an observation of the *volume* of gas, to which the former chemists entirely trusted, but which Mr Dalton seems to have wholly neglected. A still greater objection to Mr Dalton's experiment arises from the *pulverized* state in which the charcoal was employed, by which its porosity, the very circumstance essential to the success of the experiment, was destroyed ;—a circumstance, however, which he has entirely overlooked. Neither will his method of accounting for the gases afforded by charcoal, suffice for the present occasion ; since water and charcoal could only furnish gases composed of oxygen, hydrogen, and carbon, but could never explain the production of muriatic and many other gases, which his predecessors obtained. Lastly, in asserting that, in these experiments, gases cannot be expelled at a low temperature, because in the experiments of Allen and Pepys they were afforded only at a red heat, Mr Dalton, as we suppose, has confounded together things which, in themselves, are perfectly distinct ; for we know no instance in which those chemists have attempted to expel from charcoal the various gases with which it had been artificially impregnated ; and yielding, therefore, entire credit to their results obtained from charcoal under ordinary circumstances, no inference can thence be drawn against the results of other experiments, in which the circumstances are totally dissimilar. For these reasons, we do not consider the experiment of Mr Dalton as, in the smallest degree, invalidating those before related ; and if it be considered to prove any thing, it can only prove

that this attraction and condensation of gases is not effected by *pulverized* charcoal, just as M. de Saussure found the attractive power of leaves to be destroyed (342.) when they were beaten to a pulp.

614. If, then, it appear, from the foregoing facts, that air adheres to the surfaces of all bodies; that animal substances possess a strong attraction for gaseous fluids; and that the air, received into the lungs, is spread over a surface ten times greater than that of the whole body; may we not reasonably presume, that such an attractive force, operating to so great an extent, may constrain the elasticity of the inspired air, and, like the porous structure of charcoal, considerably reduce its volume? If such or any similar cause be allowed to act, it would follow, that, although the lungs do not contain a greater bulk of air than has been commonly supposed, yet that, in fact, they contain a greater weight of that elastic fluid; and hence, when any portion of this air is expelled, it will occupy a greater space than before, by escaping from the constraint to which it was previously subjected. This attractive force between the air and the animal substance, may, also, be exerted by different gases; and hence, as in the foregoing experiments of Allen and Pepys, oxygen, nitrogen, and hydrogen gases may be successively attracted and retained in nearly equal proportions, and thereby, in turn, produce each other's expulsion.

615. This supposed reduction of the elasticity of the air in the lungs does not stand in opposition either to anatomical or chemical principles; for it is considered to take place only at the surface of the

bronchial cells, and no air is supposed either to enter their absorbent vessels, or to penetrate their substance. It may not be easy to assign the immediate cause of such a phenomenon, but some facts render it probable that it may be connected with the electrical states of the atmosphere, and of the animal substance. The air of the atmosphere is known to be always more or less charged with electric matter; and in the experiments of Mr Wilson, a few blasts of air from a common bellows, especially when the pipe of the bellows was previously heated, were found sufficient to render the tourmalin, glass, and amber positively electrified*. But during respiration, the air undergoes a change in its electrical state; for Mr Read found, that the air, depraved by respiration, always indicated a negative state of electricity, while the surrounding atmosphere was in a positive state†. It is, therefore, conceivable, that, in the act of drawing off its electric matter, a reciprocal attraction may be exerted between the animal substance and the air, by which the elasticity of the latter may, for a time, be diminished; and this cause, operating over so great an extent of surface in the lungs, may possibly contribute to reduce the whole volume of air.

616. Farther, the opinion that has now been stated, presents nothing inconsistent with, or adverse to the chemical changes which the air experiences in the lungs; but, on the contrary, the union of the

* Priestley's Hist. Elec. p. 212.

† Phil. Trans. 1794, p. 267.

oxygen and carbon in those organs must be much facilitated by the more condensed state of that gas, instead of being counteracted, as it would necessarily be, by the expansion which is generally conceived to happen. Without increasing the actual dimensions, we may also, by such an hypothesis, be said really to augment the capacity of the lungs; since we compress into the same space a much larger quantity of that aëriform fluid which they are destined to receive; and, perhaps, the very different estimates which have been given of the capacity of these organs, and of the ordinary bulk of air taken in and expelled in one respiration, may receive illustration from the operation of such a cause. In the ordinary exercise of this function, however, this circumstance is not discovered, because all the causes operating on the air, either mechanically or chemically, preserve, as it were, an equilibrium of action, and, therefore, neither excess nor deficiency any where prevails; but when this equilibrium is disturbed by causes which induce a preternatural state of those organs, and change the usual relations which they maintain with the atmosphere, then the circumstance of this compressed state of the air becomes apparent.

617. Messrs Allen and Pepys farther endeavoured to determine, by their experiments, the quantity of air received into the lungs in an ordinary inspiration. In these experiments, a person in eleven minutes passed 3460 cubic inches of atmospheric air through his lungs, in which time he made about 58 respirations. And as, on ordinary occasions, this same person breathed 19 times in a minute, it is inferred,

that, by multiplying the time consumed in the experiment by the number of natural respirations in a minute, and dividing the whole bulk of air by the product, we obtain the true bulk of air received into the lungs in each natural inspiration; thus, $11 \times 19 = 209$ and $\frac{3460}{209} = 16.5$ cubic inches, which is the quantity of air this person naturally inspires*.

618. It is evident, however, that the respiration of air, under the circumstances of this experiment, can with no kind of propriety be considered as a just measure of the quantity naturally inspired; for not only do the efforts of the mind, and the operations of the apparatus, interfere greatly with the natural actions of the respiratory organs, but the gross quantity of air received in 58 preternatural inspirations, can never with justice be assumed as a true measure of the quantity breathed in 209 natural respirations. On this part of the subject, we continue to think the experiments of Dr Menzies (85.) the most unexceptionable of any which have yet been made, although the quantity of 40 cubic inches, which he assigns, does appear somewhat large.

619. In their experiments on oxygen, Messrs Allen and Pepys found, that a larger quantity of carbonic acid was formed when that gas was respired than when common air was breathed, being, in the former case, upwards of 37 cubic inches *per* minute †, and in the latter only 27. This result is directly opposed to those obtained by Mr Davy (128.), and is not, in

* Phil. Trans. an. 1808, p. 256.

† Phil. Trans. 1808, p. 267.

all cases, in accordance with their own experience ; for, in one instance, where atmospheric air was respired for five minutes and a half, the quantity of carbonic acid is stated to have amounted to 51 cubic inches *per* minute ; and the faster respiration was performed, the more carbonic acid, it is said, was given off, and consequently the more oxygen was consumed*. Indeed, it is reasonable to expect, that, within certain limits, a variation in the condition of the respiratory organs, or in the exercise of their functions, should occasion a corresponding variation in the products obtained ; but the facts just stated shew, that, if the respiration be sufficiently rapid, as much, or more, carbonic acid can be formed from common air than from pure oxygen gas ; and therefore, the production of that acid is not so much regulated by the abundance of oxygen in the air inspired, as by some other essential condition of the animal organs.

620. With respect to the actual consumption of oxygen, these chemists conclude, that the atmospheric air, expelled from the lungs, usually contains from 8 to 8.5 *per cent.* of carbonic acid, and that the proportion of acid in no case exceeds ten *per cent.* They estimate the quantity of acid thrown off in eleven minutes at 302 cubic inches, which is about 27.45 *per* minute ; and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains ; the carbon in which is 5363 grains, or rather more than eleven ounces troy. The oxy-

* Phil. Trans. 1808, p. 257.

gen consumed in the same time, will be equal in volume to the carbonic acid gas ; but it is evident that the quantity of acid expelled will depend much upon the circumstances under which respiration is performed *. This estimate falls considerably below that given by Lavoisier, Menzies, and Davy (126.) ; and we must be allowed to observe, that, although we give entire credit to the accuracy and correctness of these experiments, in determining the actual chemical changes induced by respiration on the air, yet, from many circumstances which have been already stated, we do not think them equally calculated to ascertain the quantities of air consumed in the natural exercise of this function.

621. From the foregoing series of facts, concerning the respiration of the higher classes of animals, we feel ourselves entitled to repeat with increased confidence, that “ the whole of the oxygen gas which disappears in respiration is employed (123.) to form the carbonic acid produced in that process :” And that “ the nitrogen gas of the air neither suffers any change itself (129.), nor produces any direct operation on the animal system.” Or, in the words of Messrs Allen and Pepys, “ When atmospheric air alone is respired, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas for an equal volume of oxygen †.” Consequently in man, as well as in the lower animals,

* Phil. Trans. 1807, p. 279, 280.

† Phil. Trans. 1809, p. 427.

the conversion of oxygen gas into carbonic acid constitutes the only essential change, which the air of our atmosphere experiences in the lungs during its respiration.

ADDITIONS TO CHAP. V.

OF THE SOURCE OF THE CARBON IN VEGETABLES AND
ANIMALS BY WHICH THE CHANGES IN THE AIR ARE
EFFECTED.

622. **I**N treating of the immediate source of the carbon in living vegetables and animals, and of the mode in which it was yielded by them to combine with the oxygen gas of the air, during the exercise of the respiratory function, we endeavoured to shew that its emission was connected with the exertion of a living action; and that it was not simply abstracted from the plant and animal by the operation of any affinity subsisting between it and the oxygen with which it combined. We therefore necessarily concluded, that the carbon was an excretion from the living body, depending, like other excretions, on the motion of the circulating fluids; and ceasing to be afforded, when those fluids ceased to move. Although this part of our Inquiry has appeared to attract the least attention, yet, to ourselves, it has always seemed

among the most important: and its immediate connection with our subsequent views of the theory of this function will, we hope, justify our attempt to give it all the additional evidence and illustration in our power.

623. Commencing with the seed, we endeavoured to shew (137.) that the carbon which it afforded to form carbonic acid, escaped from it in combination with the exhaled moisture, and therefore that the union of this carbon with the surrounding oxygen did not take place while it properly formed a part of the vegetable substance. In support of this opinion, we alleged that the presence of oxygen gas was not necessary to abstract the carbon of seeds (133.); and we related some experiments of M. Huber, in which carbon was given out by seeds, and combined with the nitrogen of the atmosphere, after all its oxygen was consumed.

624. Since that period, we have, in part, repeated these experiments, by placing successive quantities of steeped peas to germinate in atmospheric air, where they remained some time after all its oxygen was consumed. The residual air was then washed in lime-water to remove its carbonic acid, and it was afterwards mixed with an equal bulk of atmospheric air, previously deprived, also, of its small portion of carbonic acid. With this aëriform mixture a large phial was filled, in the usual manner, under water, and closely stopped. It was then kept inverted in water for several days, and a portion of it being afterwards passed into pure lime-water occasioned in it a slight, but very sensible discolouration. This

experiment was repeated, and afforded the same results.

625. We next varied the experiment, by mixing together equal quantities of the residual air in which beans had grown, and of oxygen gas obtained from oxide of manganese. Both the gases were first washed separately in lime-water, and were contained in a jar of the capacity of about 37 cubic inches. A small egg-cup, holding a cubic inch of the water of potassa, and covered with a piece of oiled cloth to prevent the entrance of water, was next passed under water into the jar; and the oiled cloth was immediately withdrawn. The jar with the alkaline solution was next removed from the trough on a saucer conveyed beneath it. By the tenth day, the volume of air had somewhat diminished, and the jar being now raised, the alkaline solution was passed into a tube filled with mercury, and inverted in a bason of that fluid. Diluted acid was next passed up into the tube, which excited effervescence in the solution, and produced the disengagement of about $\frac{1}{10}$ th of a cubic inch of gas, which was afterwards attracted, with the usual phenomena, by lime-water. In these experiments, the carbonic acid could have been formed only by the union of the oxygen gas with the carbon which had previously combined with the nitrogen of the atmosphere during the process of germination; and the results, therefore, accord completely with those obtained by M. Huber.

626. This expulsion or emission of carbon by seeds is farther confirmed by the experience of M. Senebier, who observed similar phenomena to occur in the

vegetation of plants. If, says he, plants be placed to grow in a vessel of common air, the whole of its oxygenous portion will be gradually changed into carbonic acid; and if the residual air be afterwards washed in lime-water, nothing remains but carburetted nitrogen gas; at least, he adds, if oxygen gas be added to this residual air, we obtain a fresh portion of carbonic acid gas. If, at the commencement of the experiment, pure hydrogen gas has been employed instead of nitrogen, and the residual air, after being washed, be then mixed with oxygen, the formation of carbonic acid is rendered still more sensible, by electrizing the aëriform mixture over lime-water, as the acid gas is then at once produced, and attracted by the lime-water*.

627. M. de Saussure, also, relates an experiment from which a similar inference may be drawn. He placed a plant of *lythrum salicaria* in 60 cubic inches of hydrogen gas, and exposed it to the sun, where, like some other plants of the same genus, it vegetated for five weeks. On examining the air at this period, it was found not to suffer any diminution by the addition of nitrous gas, neither did it contain any carbonic acid; but when a due proportion of oxygen was added to it, and it was then fired by the electric spark, it yielded water, together with portions of carbonic acid and nitrogen gases. Another quantity of hydrogen gas, which stood by the side of the former, but in which no plants had been placed, did

* Physiologie Veg. tom. iii. p. 116.

not afford any sensible quantity of carbonic acid by its combustion with oxygen gas *. The hydrogen gas, in the former case, must have derived its carbon from the plant, and this carbon, during the combustion, must have united with the oxygen to form carbonic acid. The production of water arose, no doubt, from the direct union of oxygen and hydrogen, while the nitrogen proceeded from the air previously existing in the plant.

628. Still farther to corroborate these results, we added to about 22 cubic inches of the residual air of vegetation 14 inches of oxygen, after both gases had been first washed in lime water. Water of potassa was then passed into the jar, in the manner before related (625.), and the jar was thus completely filled by the aëriform mixture and solution. A saucer, filled with mercury, was then passed under the jar, which was thus removed from the trough, and the water on the outside of the jar was removed by a sponge, and only a thin film of that fluid covered the mercury within. By the 16th day, the mercury had risen nearly four-tenths of an inch into the jar; and when, at this period, the jar was raised, and a diluted acid poured on the alkaline solution, it excited in it a very brisk effevescence. These facts sufficiently testify that carbon is given out both by seeds and plants in a form capable of uniting either with nitrogen or hydrogen gas; and they consequently prove, that the presence of oxygen gas is not necessary to abstract this substance.

* Recherches Chim. sur la Vegetation, p. 211.

629. But if hydrogen and nitrogen thus enter into union with the carbon of vegetables, in what place and mode are we to consider this union to be accomplished? Are we entitled to suppose that these gases penetrate the organized structure of the seed and plant, and are afterwards expelled from them, charged with carbon? If such a supposition be received, the operation must be effected by the agency of a living power, or by the exertion of chemical affinity. Against the former, it may, however, be urged, that vegetables, when confined in these gases, do not exhibit the phenomena of living action; and consequently we possess no evidence that such action is then exerted.

630. Shall we then ascribe this supposed entrance of these gases into vegetables to the operation of chemical affinity? That the actual union of these bodies is brought about by the exertion of such a force, we readily grant; but our present object is to discover the manner in which these elements are prepared for such an union, and the place where this union is accomplished. Now, before carburetted nitrogen or hydrogen was formed in these experiments, the carbon existed in combination with the other elements of the seed; and from those elements it must have separated, before it could combine anew. Was then this separation effected by a superior attractive force exerted by these gases, or did it proceed, in the first instance, from a diminished attraction, or incipient repulsion among the constituent elements of the vegetable? We adopt the latter view, and, consequently, suppose the carbon to be given off by the seed or

plant, rather than believe it to be drawn away by the attractive force of the gases to which it was exposed.

631. If we may be allowed to venture an opinion upon a difficult question in chemical doctrine, we would beg to remark, that chemists appear to have ascribed too much to the attractive, and too little to the repulsive forces, exerted between the particles of bodies. In artificial experiments, they generally effect decomposition by the employment of agents which exert a superior attractive force towards one or other of the elements of the compound ; and the decomposition may then be truly said to be accomplished by the energy of attraction. But decomposition occurs, likewise, spontaneously in bodies, where no particular attraction is exerted by the surrounding bodies, and where, therefore, the first movements towards change must arise in the bodies themselves. By these spontaneous movements, the composition of the existing compound is subverted, and its separated elements are then at liberty to enter into new combinations with each other, or with the bodies which surround them. Hence the decomposition of a body may be effected either by the superior attraction exerted towards one or more of its elements by some other body ; or it may proceed from a spontaneous change in the natural affinities of the elements of the body itself, in which case the separation of its parts may more properly be said to arise from repulsion than from attraction.

632. All the phenomena of spontaneous decomposition appear to us, indeed, to suggest the belief, that

the elements of the compound gradually pass from an attractive to a repulsive state, by which its original constitution is subverted. Thus, when carburetted hydrogen or nitrogen, or even carbonic acid, is formed by seeds placed either in hydrogen, nitrogen, or oxygen gas, we see no reason for supposing these gases to penetrate the substance of the seed, and, by a superior affinity, to draw off its carbon; but we rather suppose this carbon, by a previous change in the seed, to be released from its former state of combination, and brought into a condition more or less fitted to combine with the gaseous bodies that surround it, according to the order of their affinities for that substance.

633. Perhaps the expulsion or emission of carbon, under such circumstances, may receive illustration from the emission of odorous particles from bodies. It will not, we conceive, be said that the emission of these particles is caused by the attractive force exerted by the elements of the atmosphere, but that it proceeds from some spontaneous changes going on in the body itself, the atmosphere serving only as a vehicle for the conveyance of these particles, and as a medium which readily permits their escape and diffusion. If, however, these bodies be placed in certain circumstances, the emission of their odorous particles is more or less checked and prevented. This may be accomplished either by withdrawing heat or moisture, both of which were necessary to excite and facilitate those spontaneous changes on which the emission of the odour depended; or the odour may be arrested in its escape by mechanically stopping up the pores

of the body with varnish or oil. Should this, however, be done, the emission of the odorous particles cannot be said to be prevented by the absence of any cause required to attract them, but by the removal of those circumstances which were necessary to favour their separation, or by the interposition of other bodies which prevent their escape, even although their separation be made.

634. In like manner, we consider the separation of carbon from the other elements of seeds, so far to resemble these operations, that, at the commencement of germination, it is effected by the spontaneous changes which arise in the seed, and not by the attractive force of the oxygen of the air. When, however, oxygen gas is supplied, its superior affinity for the carbon of the seed leads to its combination with it, to the exclusion of nitrogen gas ; and, by a subsequent agency, arising out of that combination, the development of the seed is accomplished. Although, however, oxygen gas be necessary to this particular change in the seed, we are not from thence to conclude that it is necessary to every change ; for we have seen that the seed experiences other changes when no oxygen gas is present. Hence, therefore, it appears to us that the carbon yielded by seeds, both under decomposition and in germination, proceeds, primarily, from spontaneous changes excited in the seed itself, and that it is not removed by the operation of an attractive force, exerted by the surrounding oxygen.

635. What, then, are the circumstances or agents which favour or promote that spontaneous change in

the seed which leads to the separation of one or more of its constituent parts? They are essentially heat and moisture; for it is only when the seed is duly moistened, and placed in a proper temperature, that, either by decomposition or by germination, it is able to emit carbon. By a high degree of heat alone, indeed, the carbon of the seed may be separated and expelled; but the results are then altogether different from those which follow the spontaneous changes to which we have alluded, and to which the concurrence of moisture is necessary. Since, therefore, heat cannot be the sole agent by which the carbon of the seed is brought into a state capable of combining with the oxygen gas of the air, no other mode occurs to us in which this can be accomplished, but that of solution in water; and, therefore, we conclude that this carbon passes off with the moisture of the seed, or is, at least, brought into a state capable of doing so by its solution in that fluid.

636. To this conclusion it has been objected, that carbon is not soluble in water, because pure charcoal appears to resist the solvent power of that fluid. However much this objection may weigh against the solubility of pure carbon, it does not apply to carbon as it exists in vegetables, where the complex state of its combination may greatly facilitate its solution. For "in chemistry," says Dr Thomson, "it may be considered as a truth, to which at present few or no exceptions are known, that bodies are decomposed with a facility inversely as the simplicity of their composition; those which consist of the fewest ingredients being decomposed with the greatest diffi-

culty, and those which are formed of many ingredients, with the greatest facility *." In proof of the solubility of carbon, it may be added, that there is scarcely any vegetable fluid, either natural or artificial, which does not contain carbon in a state capable of forming carbonic acid by combining with the oxygen of the air. Thus it is known that the infusion of malt, in the process of brewing, produces an abundance of carbonic acid; and M. de Saussure observed, that vegetable juices and extracts dissolved in water, and vegetable infusions in passing to the state of vinegar, converted the oxygen gas of the air into an equal bulk of carbonic acid †.

637. It has been farther objected, that the union of carbon and oxygen cannot be effected at so low a temperature as is here supposed; but, unless we deny altogether the formation of carbonic acid in decomposition, as well as in germination, we must admit such combination to take place at the ordinary atmospheric temperature. It is well observed by Mr Murray, that, "although carbon, in its solid and insulated form, requires to be raised to a high temperature to cause its combination with oxygen, yet, when it makes part of a ternary or quaternary combination, in which state its cohesion no longer opposes the combination, it then combines with oxygen at any natural temperature ‡."

* Thomson's Syst. Chem. vol. v. p. 417. 3d edit.

† Recherches Chim. p. 140, 144.

‡ Syst. Chem. vol. iv. p. 565.

638. We have no grounds to believe that the carbon of vegetables, in combining with oxygen, passes off in a gaseous form; for although, in the compounds of carbonic acid, and of carburetted hydrogen and nitrogen, it exists in an elastic state, yet there is no evidence of its being able to maintain that state, unless it be combined with some permanently elastic body; and when, by decomposition, it is again separated from such combinations, it resumes a solid form. Thus Mr Tennant found charcoal to be precipitated in the decomposition of carbonic acid by phosphorus (464.); and Mr Cruickshank and Dr Henry, on submitting pure carburetted hydrogen and olefiant gases to electrization, observed the charcoal to be separated and deposited on the inner surface of the glass-tube, and the hydrogen then assumed a state of greater expansion *. As, therefore, this substance does not seem capable of existing by itself in an elastic form, like some other inflammable bases, we have additional grounds for concluding that it really passes off from the seed in solution, or in combination with water.

639. But while we thus suppose that carbon is rather given off by the seed or plant to unite with the surrounding gases, than that these gases are first attracted into the seed to combine with and carry off its carbon, we do not mean to deny that oxygen, and even other gases may not exert attractions towards the vegetable body, and, by their mechanical or

* Phil. Trans. 1809, p. 448.

chemical properties, penetrate, in a certain degree, the pores of the seed, and there unite with its carbon, after it has been brought into a state fitted for such combination by the spontaneous changes described above. All that we contend for, is, that the separation of the carbonic matter from the other elements of the seed is not effected by the attractive force of the oxygen, but that the seed itself must have previously undergone changes, by which its carbon is reduced to a state fitted to combine with the surrounding air; and so far is oxygen from being necessary to abstract this carbon, that this substance is often given off by the seed where no oxygen gas is present. In ordinary cases, therefore, we believe the union of carbon and oxygen to take place at the surface, or, it may be, within the pores of the seed or plant; but if oxygen be not supplied in due quantity, to unite with and carry off the carbon, this substance will escape, in combination with nitrogen or any gas that surrounds it, according to the laws of its affinity for it; or, if no gas be present, it then passes off in an elastic form in union with oxygen (19.), derived either from the decomposition of the seed, or of the water which it has previously imbibed. Even in hydrogen and nitrogen gases, carbon not only passes off in a simple form, but also in combination with oxygen (5.), derived from one or other of these sources.

640. Without attending to the state in which we have supposed the carbon to pass off from living bodies, some writers have imagined that we considered it to be afforded in a solid form. It has, however, been our aim throughout to establish the existence

of this substance in a fluid state; and we have no objection even to admit of its gaseous escape from bodies, if any evidence can be brought of its capacity to maintain that state, or if it can be shewn, by experiment, to escape in combination with any other permanently elastic matter; but, possessing no evidence that it really does so, and feeling assured that it actually is held in solution in water, we conclude in favour of the latter opinion. Hence we would not be understood to attach any new or peculiar notion to the mode or state in which carbon exists; but to speak of it as a solid, fluid, or gaseous substance, according as it may chance to exist in one or other of those states.

641. As, in the preceding discussion, we have spoken indiscriminately of the emission and combination of carbon with oxygen gas, both under the decomposition of organised bodies and during the existence of living action, it may be proper that we should state our opinion of the difference, although we are not able to define the exact limits of those states or terms. Thus we have seen, that carbon is afforded, in union with oxygen, by seeds confined in hydrogen or nitrogen gases (5.), or even in pure mercury (19.), where nothing resembling a living action can be supposed to exist. The same conditions of heat and moisture which, in these circumstances, enable it to yield carbon, are required, also, for the emission of this substance during germination; but no development takes place, unless oxygen gas be likewise present. Now the carbon, at this early period of the process, we suppose to be gi-

ven off by the operation of the same causes, and nearly in the same manner as it is afforded by inanimate bodies. By this carbon the surrounding oxygen is changed, and the development of the seed succeeds to this chemical action. In the progress of this development, the organization of the seed is unfolded; and when this is sufficiently complete, the emission of carbon, like the other functions of the seed, is then executed by an organised structure, and becomes obedient to those laws which govern and regulate the actions of living beings. It is the same with regard to moisture. The dried seed imbibes moisture, at first, like any other inanimate body. By this moisture, in conjunction with other agents, its organic structure is gradually developed, whereby a vascular system is evolved, and then properly commences the function of absorption. Hence, therefore, as it may be said that seeds, at first, simply imbibe and afterwards absorb moisture, so do we say that the carbon at first is afforded by them as it is by inanimate matter; but subsequently it passes off through an organic structure, when it may properly be said to be excreted or exhaled.

642. In support of the doctrine, that the carbon of vegetables passes off in combination with their exhaled fluids, we farther maintained (139.), that the changes in the air were most extensively effected, when plants possessed the greatest vigour, and the motion of their fluids was most actively carried on. On the contrary, whatever arrested the motion of these fluids, diminished the extent of the changes in the air, and consequently bore witness to a diminish-

ed exhalation of carbon. It is in conformity with this view that evergreens, which perspire less (23.) than other plants, were found by M. de Saussure to form less carbonic acid*; and when the perspiratory function wholly ceases, there is reason to conclude that no farther change is then induced on the air.

643. But the leaves of plants, like other organic substances, are enabled to afford carbon after all living action has ceased, provided they be placed in those circumstances of moisture and heat which give rise to the spontaneous changes necessary to the performance of this chemical action. Hence plants, like seeds, form carburetted hydrogen when confined in vessels of that gas; and their leaves, after separation from the stem, deteriorate the air if they are kept in the shade†. In the progress of their decomposition, also, either in water or in air, their carbon escapes in combination with different elastic fluids. From these facts it appears, that, in due degrees of moisture and of heat, a constant action is maintained between vegetables and the air, under which carbonic acid is produced. In the dead plant, however, this carbon is separated in the progress of those spontaneous changes which terminate in the destruction of the vegetable structure: in the living one, it is given off as an excretion, and therefore depends primarily on those laws and conditions which regulate the motions and conditions of the vegetable fluids. In many instances, however, it may be diffi-

* *Recherches Chim.* p. 96.

† *Ibid.* p. 60.

cult, or even impossible, to fix the precise limits where the one operation terminates, and the other begins.

644. With respect to the *ulterior* source of the carbon in living vegetables, we have supposed (159.) that its supply, in the later periods of vegetation, is derived, like the other ingredients which go to the composition of the plant, from the soil or situation in which it may be placed to grow. From the fact, however, of carbonic acid being decomposed by plants which vegetate in sunshine, it has been inferred by M. M. Senebier and De Saussure, that the carbon of this acid was retained and assimilated so as to afford a constant supply of nutriment to the plant. But plants have been shewn to live and grow in the shade, where they decompose little carbonic acid, and can, therefore, in this manner, derive but little carbon; and, even in sunshine, it has been also shewn (305.) that they grow for long periods in atmospheric air without affecting permanently either its purity or its volume. Consequently, if it were granted, that, by the decomposition of carbonic acid, they obtained carbon in sunshine, they must again give out an equal quantity of it, when they form carbonic acid in the shade; so that no excess of carbon remains to be applied to the growth and augmentation of the plant. M. de Saussure, indeed, admits that plants grow in atmospheres perfectly freed from carbonic acid; but he then supposes them to obtain carbon by decomposing the acid which they had previously formed. It is, however, obvious, that they cannot, in this way, absolutely acquire carbon;

since, as has been well observed, this acid could not furnish to plants more of its base than it had previously taken from them *. When, too, we consider the almost inappreciable quantity of carbonic acid that exists in the atmosphere, and call to mind that its proportion remains uniformly the same, whether vegetation continue or be suspended, we cannot bring ourselves to believe, setting aside all physiological objections to this mode of nutrition, that plants obtain from the atmosphere any portion whatever of that carbon which administers to their nutrition and growth.

645. From the following experiment of M. Braconnot we learn, likewise, that plants, during their vegetation, may considerably augment their proportion of carbonic matter in situations in which no carbon can be supposed to be derived from the atmosphere that surrounds them. He confined 460 seeds of white mustard in a large glass bottle, filled in part with very fine white sand, which was previously deprived of all calcareous earth by washing it in muriatic acid. This sand he then moistened with distilled water, and filled the remainder of the bottle with atmospheric air, freed from carbonic acid. After closing the bottles very accurately, they were placed a few inches deep in a moist soil. Vegetation soon commenced, and considerable verdure was produced. At the end of six weeks, the plants were taken out of the bottle, and washed with great care, and dried. In this state,

* Nicholson's Journal, vol. xviii. p. 22.

they weighed 140 grains, and after the saline and other matters were driven off by a strong heat, there remained 23 grains of pure carbon; while 460 seeds of the same kind, which were submitted to a similar process of incineration, afforded much less carbon; so that the seeds in close vessels acquired, says M. Braconnot, $15\frac{1}{2}$ grains of pure carbon, which appeared evidently to have been formed at the expence of water, and probably of light*. The air, he adds, had undergone little change; which, doubtless, arose from its being so fully exposed to the agency of light.

646. The foregoing result clearly shews that plants acquire carbon in air which contains no carbonic acid; but on its absolute correctness we are disposed to place but little reliance. The attempts to ascertain the quantity of carbon in vegetables, by the processes of distillation and combustion, in the manner they have been hitherto conducted, appear to us very unsatisfactory; for no account seems to have been taken of the quantity of that substance which, during the operation, passes off in a gaseous form. "When, however, a vegetable substance, composed of oxygen, hydrogen and carbon, united in the form of a ternary compound, is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities, which constituted the triple combination, says Dr Henry, is destroyed; and the elements composing it are united in a new manner. Those which are disposed to enter into permanently

* Nicholson's Journal, vol. xviii. p. 25.

elastic combinations escape in the state of gas; and thus the aëriform products of the distillation of vegetable substances are mixtures of carbonic acid, carbonic oxide, olefiant, carburetted hydrogen, and simple hydrogen gases; or of two or more of these in various proportions *." Until these sources of fallacy be duly attended to, and fairly estimated, it is in vain to place any great confidence in the attempts to ascertain the quantity of carbon in plants by the destructive mode of analysis that has now been stated.

647. But M. de Saussure has attempted to shew, by arguments of a somewhat different kind, that growing vegetables derive carbon from the atmosphere. M. Hassenfratz had before endeavoured to prove, that plants which grew in pure water, and were exposed to the atmosphere, contained less carbonic matter than the seeds from which they sprang. M. de Saussure obtained a similar result when he caused plants to grow in places weakly illuminated; and the absence of light, therefore, may, he supposes, account for the results which M. Hassenfratz obtained †. In other experiments, however, conducted under exposure to the sun, different results were afforded. The roots of several plants of peppermint were plunged in phials of distilled water, and left to vegetate, in a free exposition to the sun and air, but protected from rain. After growing ten weeks, 100 parts by weight were increased to 213 parts.

* Phil. Trans. 1808, p. 283.

† Recherches Chim. p. 52.

These 216 parts were then reduced, by drying them at the temperature of the atmosphere, to 62 parts, which afforded, by incineration, 15.78 parts of carbon. At the same time that the plants above mentioned were put to grow in distilled water, an equal weight of similar plants was dried and incinerated in the same manner, but they yielded only 40.20 parts of dry vegetable matter, which contained only 10.96 parts of carbon; whence it is concluded, that the plants which had grown ten weeks in the open air, and were supplied only with distilled water, had acquired 4.82 parts more carbon than they possessed before the experiment.

648. In another instance, M. de Saussure placed four beans, weighing 120 grains, to vegetate in pure silicious earth contained in a glass capsule. They were watered with distilled water, and kept, for three months, in a free exposition to the sun and air. When taken up green, immediately after flowering, they weighed 1642 grains, but were reduced, by desiccation, to 202 grains, which afforded 51 grains of carbon; while four similar seeds, of the same weight, and dried and carbonized in the same manner, yielded only twenty-two and a half grains of carbon. Hence, says M. de Saussure, the first four beans had more than doubled the quantity of carbonaceous matter by vegetating in distilled water in the open air; and it cannot be doubted but that this matter was derived from the decomposition of carbonic acid found in the atmosphere*.

* Recherches Chim. p. 50. et seq.

649. Even although we pass over the sources of fallacy in estimating the quantity of carbon already stated, and yield entire credit to the accuracy of the results in these comparative experiments, yet many circumstances concur to prevent our acquiescence in the conclusion drawn from them. For, first, we have no evidence that the small portion of carbonic acid, contained in the common atmosphere, can enter plants in order to be thus decomposed and yield its carbon, since the results afforded by plants growing in closed vessels cannot, in this instance, be justly applied to those obtained under a free exposure to the air, where the proportion of carbonic acid is so much smaller, and its ready diffusion through the atmosphere must so rapidly take place. By similar experiments, indeed, M. de Crell was led to an opposite conclusion; for the quantity of carbon, contained in the carbonic acid of the atmosphere, could not, in his opinion, account for the addition of that substance which the plants, in his experiments, received; and he, therefore, supposed plants to possess the power of composing carbon, employing for this purpose only water, atmospheric air, and light*. M. Braconnot, also, by experiments of the same kind, has been led to conclude that vegetables find in pure water every thing necessary for them to assimilate; that vegetable mould and manures yield no nutriment, but are useful only as they improve the texture of soils, and retain and supply moisture; and that earths, alkalis, metals, sulphur, phosphorus,

* Phil. Mag. vol. xxiv. p. 150.

and charcoal, are developed from water by the organic powers of plants, assisted by solar light *.

650. The discoveries of Mr Davy, however, which are scarcely more to be valued for the actual additions they have made to the sum of our knowledge, than for the corrections they have introduced into what we were before supposed to know, have disclosed new sources of fallacy in these experiments, which the state of science, at the period they were made, could not have enabled their authors to foresee. "The experiments," says he, "in which it is said that alkalies, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner; for distilled water may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds."—"The conclusions of M. Braconnot," he adds, "are rendered of little avail in consequence of these circumstances. In the only case of vegetation in which the free atmosphere, in his experiments, was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances which might afford carbon or various inflammable matters."

651. "In the common processes of nature," continues Mr Davy, "all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of

* Nicholson's Journal, vol. xviii. p. 27.

the alkalies, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic, and granitic rocks, there is a constant supply of earthy, alkaline, and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutro-saline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements *." To these views of the economy of living beings we yield our cordial assent, and hold them to be not less consistent with the most advanced state of chemical science, than with the justest conceptions we can form of the varying structure and properties of organic beings. They lead us directly back to the opinion, that vegetables derive the carbonaceous matter that contributes to their growth through the fluids which they absorb from the situations in which they grow.

652. IN the same manner as the carbonic matter is exhaled by plants, so likewise have we maintained that, during the continuance of living action, it is given off by the exhalent function of animals. The facts which we adduced in support of this opinion

* Phil. Trans. 1808, p. 33, 34.

were chiefly of a physiological nature ; and although they do not seem to have drawn much attention from others, yet to ourselves they have always appeared to approach nearly to perfect demonstration. They were derived chiefly from the “Memoirs on Respiration” by the late Abbé Spallanzani, and the continuation of this writer’s labours enables us to add some facts in addition to those already stated (142.), in farther confirmation of this doctrine. Whatever difference of opinion may prevail as to the state in which the carbon exists, and the mode in which it is expelled from the body, we must think the following facts afford decisive evidence of the immediate dependence of this action upon the *living powers* of the animal system.

653. It has been already remarked (1.) that seeds, in a perfectly dry state, do not, in the smallest degree, affect the quality of the air in which they are confined ; and such, too, we may conclude, must be the case with the rotifer (48.) and various other zoophytes, which, when rendered perfectly dry, remain unchanged for an indefinite period of time.

654. The experiments already detailed (142.) sufficiently prove, that, during the suspension of living action in the vermes and mollusca classes, no change whatever is induced on the air that surrounds them. The correctness of these facts we have since verified by experiment ; for we found that snails, while confined in glass vessels over mercury, and kept in temperatures at or below the freezing point, remained quite torpid, and did not emit any sensible portion of fluid from their bodies, nor, in the smallest de-

gree, affect the quality of the surrounding air ; but if the temperature was raised a few degrees, the vessel soon became dim from the exhalation of fluid, the animals revived, and the oxygenous portion of the air was then, as usual, converted into carbonic acid. We have before related examples of snails (48.) which, like the rotifer, were rendered torpid by the abstraction of water ; and, since they remained in that condition for years without exhibiting any material change, it may be safely inferred, that, during the same period, they effected no change in the air that surrounded them.

655. In the insect class, Spallanzani found that caterpillars, which were in full vigour, consumed more oxygen and produced more carbonic acid than others ; and when about to change into chrysalides, they consumed less than in their caterpillar state *. A caterpillar, confined in air at temperature 2° Reaumur, consumed, in five hours, only 0.02 of its oxygen gas, while a similar caterpillar, kept for the same time in a temperature varying from 16 to 17° , consumed 0.08 of oxygen gas. When several caterpillars were kept for a whole night in a vessel of air, preserved in a temperature at and below zero, the air, on examination, contained its usual quantity of oxygen, so that no portion of that gas had disappeared. When, however, the temperature was raised to $1\frac{1}{2}^{\circ}$ above zero, the animals then, in a small degree, consumed the oxygen gas of the air †.

* Rapports de l'air avec les êtres organisés, tom. i. p. 25.

† Ibid. p. 30, 31.

656. With respect to fishes, Spallanzani remarks, that, in great colds, they bury themselves in holes, and in the slimy beds of streams; but never become absolutely lethargic *. Some tenches, which were placed in water five degrees below zero, continued to move although the water froze around them. In cold air they soon became immoveable, but instantly recovered action when replaced in water †. In these low temperatures, they breathed only seven times in a minute; but as the temperature was increased, the respiration became accelerated. After a certain time, however, they died in vessels of frozen water from the want of fresh air ‡. A tench, kept during a whole night in air that was only half a degree above zero, consumed a small portion of its oxygen and formed carbonic acid; which is a farther proof that they do not become entirely lethargic from cold §. From these facts we learn, that fishes do not naturally become lethargic from cold, and therefore continue always, in a small degree, to act upon the air. They lose, however, their former vivacity, and, in proportion as their respiration is suspended, they consume less oxygen gas ¶.

657. In the reptile class, the same author observed, that, in a temperature one degree and a half below zero, the heart in serpents beat only twice in a minute, and respiration was suspended. When removed to a temperature of 7° , the heart soon recovered its action, and beat 10 or 12 times in a minute; and

* Rappports, &c. tom. i. p. 157.

† Ibid. p. 158, 161.

‡ Ibid. p. 163.

§ Ibid. p. 165, 168.

¶ Ibid. p. 186.

in still higher temperatures, the pulsations amounted to 28 or 30 *per minute* *. From these and similar experiments, Spallanzani concluded that cold diminished the action of the heart and lungs, and, when the lethargy was complete, the motion of the heart almost entirely ceased, and very little effect was then induced on the air †. In like manner, frogs, and other animals of this class, became more or less lethargic under great reductions of temperature; but the heart continued to beat slowly, and the air, in a small degree, was changed; the consumption of oxygen, however, was always proportional to the elevation of temperature ‡. These facts prove, that, in all the lower animals, the consumption of oxygen gas by respiration is preceded by the motion of the animal fluids, which again immediately depends on the presence and operation of heat.

658. No experiments, we believe, have yet been made on the respiratory process of birds during their torpid state; but if the assertion, that these animals often pass the period of hybernation under water, be true, they must retain the faculty of reviviscence in situations from which air is excluded. As, therefore, in such situations, air is not necessary to their existence, it may be reasonably inferred, that when, in other situations, the actions of life are suspended, the air does not experience any chemical change.

659. In the mammalia class, we have already adduced (143.) the example of the marmot as com-

* Rappports, &c. tom. i. p. 230.

† Ibid. p. 250.

‡ Ibid. p. 470.

pletely proving the fact, that no chemical change is effected in the air by the respiratory organs, during the continuance of complete torpor; and Spallanzani found, also, that neither bats, dormice, nor hedgehogs, produced any change in atmospheric air during the suspension of the circulating and respiratory functions. The results thus obtained, by experiments on the lower animals, may be extended, in all their circumstances, to man; for although we possess no direct proof of the air remaining unchanged during the suspension of respiration, yet the actual cessation of that function may be regarded as evidence of it; and we have also seen that man, like other animals, consumes more oxygen while he is vigorous, and during a state of exertion, than under the opposite circumstances of debility and inaction.

660. But if it thus appear, that, so long as the blood, in all animals, continues to move, the oxygen gas of the air continues to be changed; and conversely, if no change be effected in the air when this motion in the blood has ceased; we must suppose some necessary connection to subsist between these events: and farther, as the change in the air follows always the motion of the blood, and gradually ceases to be produced as that motion declines and ceases, we must also suppose the effect in the air to depend on this antecedent motion of the blood, which, therefore, in common philosophical language, we are entitled to consider as its cause. In the order of events, however, this motion of the blood is not the immediate cause of the change in the air, but only enables

the animal to furnish carbon in that mode and state by which this chemical action on the air is accomplished. Consequently we conclude, that the carbon supplied by animals, during the exercise of the respiratory function, is an animal excretion, dependent, like other excretions, on the motion and distribution of the blood.

661. We before maintained that this carbon, in living animals, was brought into actual contact with the air through the medium of the exhalent vessels in the lungs, and we remarked (156.) the peculiar circumstance of this exhalation being furnished as well from the venal, as from the arterial, blood in those organs. The following observations of Dr Barclay entirely accord with these views. "The change produced in the blood by respiration," says he, "is probably owing more to exhalation than absorption, for no artery exhales so freely as the pulmonic. A watery injection thrown into this artery with a small force, will flow copiously into the bronchi, and without occasioning any thing like rupture in its smaller vessels. But whether the exhalation be, or be not, the principal cause of change in the blood, pulmonic blood, when exposed to the air through the medium of its vessels, is always observed to change its colour a great deal faster, while the exhalents continue to act with a vital energy, than when they act slowly and feebly as inanimate organs *."

* On Muscular Motion, p. 524.

662. Besides this exhalation of carbonic matter by the respiratory organs of animals, many facts tend to prove, that, during the continuance of living action, a similar change is effected in the air by the skin. Spallanzani found that some insects, which possess no stigmata, consumed the oxygen gas of the air by the medium of their skin; and the same office must be performed by the skin in many worms and zoophytes, which possess no other distinct respiratory organ. Spallanzani farther observed, that the skins of fishes effected changes in the air similar to those produced by the gills *; and M. M. Provençal and Humboldt found, likewise, that the water in which the bodies of these animals had been confined, afforded the same aëriform products as when they were permitted to breathe by their gills†. In the experiments of Spallanzani, the skins of serpents were found also to consume the oxygen of the air, and to produce carbonic acid like the lungs‡; and frogs, after being deprived of their lungs, still lived and consumed oxygen gas, which they appeared to effect by the action of their cutaneous organ§. Hence, then, the power of the skin to convert the oxygen gas of the air into carbonic acid seems to be possessed by all the lower animals.

663. From the results of various experiments by De Milly, Cruickshank, Abernethy and Jurine, we were before led to conclude (147.), that carbonic acid

* Rapports, &c. tom. i. p. 187.

† Mem. d'Arcueil, tom. ii.

‡ Rapports, &c. t. i. p. 250.

§ Ibid. p. 469.

was formed by the human skin, wheresoever the external surface of the living body came into contact with atmospheric air; but, in all the experiments made to establish this point, sources of fallacy, which render the opinion somewhat doubtful, may be pointed out. In De Milly's experiments, the water in which the body was bathed might afford carbonic acid, as Priestley (148.) and others remarked. In Mr Cruickshank's experiments, the hand and foot were confined in a vessel covered over with a moistened bladder; but this bladder, as Dr Klapp observed, might furnish carbon to unite with the oxygen gas of the air, and thus give rise to the production of carbonic acid *. The vitiation of the air in bottles fastened to different parts of the body, as reported by M. Jurine (147.), did not take place in similar experiments made by Dr Priestley †; and the carbonic acid, found by the same author, in the air confined under the bed-clothes, where different persons had slept, might, as M. Seguin remarked, proceed directly from the lungs. Lastly, the experiments of Mr Abernethy, who supposed both an absorption and transpiration of aëriform fluids to be carried on by the skin, are contradicted, in all their results, by the later and more accurate trials of Dr Klapp (150.), who found that no emanation of gas took place from the skin, when the hand was confined for several hours in hydrogen gas, in mercury, or in lime-water.

* On the Functions of the Skin, p. 14.

† Obs. on Air, vol. iv, p. 275.

664. But Dr Klapp farther contends, not only that no aëriform fluid is perspired by the skin, but that this organ does not change the quality of the air that surrounds it. He held his hand and wrist for three hours in a vessel of atmospheric air confined over mercury, and kept at the temperature of 60° ; but on analysing the air, no change was found to have taken place either in its composition or its volume. A second experiment was made with oxygen gas nearly pure, but no carbonic acid was produced; neither did any change take place in the volume of the gas *. A similar result had been previously obtained by Dr Priestley, who kept his arm for an hour in warm water, while his hand was passed up into a jar of air inverted in water; but the contained air did not appear to have suffered any alteration †.

665. These experiments, to ascertain the action of the human skin on the atmosphere, have been lately repeated, with great care, by our friend Dr Gordon, and with results similar to those which have just been stated. He kept his hand and fore-arm, for an hour, in a vessel of atmospheric air, inverted over water and heated to the temperature of 88° Fahrenheit. The arm was then withdrawn, and the air allowed to cool down to the temperature of the surrounding atmosphere. Its bulk was now found to be exactly the same as before the experiment, and a portion of it being analysed by lime-water, and by phosphorus, appeared to possess no additional quan-

* On the Functions of the Skin, p. 25.

† Obs. on Air, abridged, vol. ii, p. 195.

tity of carbonic acid, nor to have lost any part of its oxygen gas. Precisely the same results were obtained in two other trials.

666. To obviate any objection arising from the circumstance of the hand having been passed into the vessel through water, Dr Gordon next immersed his elbow to the depth of about four inches in a trough of water at temperature 65° ; so that the hand, and almost the whole of the fore-arm, remained quite dry above the surface. Over the hand and arm, a jar, filled with atmospheric air, was then cautiously inverted. When the mouth of the jar was brought into contact with the water, a portion of the air was removed by an exhausting syringe, and its place supplied by the water in the trough, so as to remove all danger of the escape of any part of the air actually employed in the experiment. During the experiment, warm cloths were kept constantly applied to the outside of the jar. The pulse beat 64 times in a minute, and the temperature under the tongue was 90° . A fine dew soon began to form on the sides of the vessel, and to trickle down in small streams. At the end of an hour, the hand was withdrawn; its surface felt warm and moist. When the included air had returned to the temperature of 60° , it was found to occupy the same bulk as at first; and, on being submitted to analysis, it yielded no carbonic acid, but afforded the same proportions of oxygen and nitrogen gases, as it possessed before the experiment. These results, therefore, accord with those obtained by Priestley and Klapp; and seem to prove not only

that no aëriform fluid, but also that no carbonic matter is exhaled by the human skin.

667. Adverse, however, as these experiments seem to the opinion that the oxygen gas of the air is affected by the human skin, yet, in others lately made by Dr Charles Mackenzie, and which we had an opportunity of witnessing, carbonic acid was clearly detected in air that had been kept for two hours in contact with the skin. Dr Mackenzie confined his hand and wrist in a glass vessel which contained about 50 cubic inches of atmospheric air. Around the mouth of the vessel a piece of oiled silk was fastened, through which the hand was introduced, and the silk cloth was then closely secured round the arm above, so as to cut off the communication with the external air. In a few minutes the inside of the vessel was bedewed with moisture, which, during the experiment, accumulated to the quantity of nearly half an ounce. The experiment was conducted in a warm room, and was continued for rather more than two hours. In order to examine the air, the hand, with the glass vessel still attached to it, was plunged under water in a pneumatic trough, and the oiled silk being then removed from the arm, the hand was withdrawn. A portion of the air was now passed from the glass vessel into a small tube filled with pure lime-water. It did not affect the lime-water in its transmission through it, but a white filmy crust formed on those parts of the sides of the tube from which the lime-water had been expelled, and in a minute or two, white threads of carbonate of lime likewise fell down through the mass of fluid. The

experiment was repeated, and with nearly the same results; but as the temperature of the room and of the body were lower than in the former experiment, the effects were not so distinct.

668. The foregoing experiments seem clearly to establish the existence of carbonic acid in air that has been kept in contact with the skin; and this acid gas must have been either formed by the action of the skin upon the surrounding air, or emitted ready formed by that organ; for no trace of carbonic acid could be discovered in the air before it was submitted to experiment. Now the experiments of Dr Klapp (663.) seem decisive against the supposition that gaseous fluids emanate from the skin; so that we are compelled to consider this acid gas as formed by the union of the oxygen of the air with the animal carbon, precisely in the same manner as it is formed by the skins of the lower animals (662.), and also by the respiratory organs of man himself. Dr Mackenzie ascertained, that, in these experiments, the carbon did not proceed from the oiled silk; for when the same silk was kept for 48 hours in contact with air, it produced in it no trace of carbonic acid.

669. The effects produced in the air by animal solids and fluids, after their removal from the living body, confirm, in all respects, the belief that they contain carbon in a state fitted to combine with oxygen gas. We have seen that both the serum of the blood (97.) and the entire mass of that fluid, convert the oxygen gas of the air into carbonic acid; and similar effects are produced by the other animal fluids. So, likewise, moistened bladders (587.) act equally

upon the air ; and, in their spontaneous decomposition, all animal substances were found by Priestley (145.) and Spallanzani * to exert a similar operation. Now, by all these animal compounds carbon must have been furnished, and the mode of its separation from the other ingredients, and of its combination with oxygen, we conceive to be similar to what has already been stated (632.) to happen in the decomposition of vegetable bodies. Certain degrees of moisture and of heat are necessary to the exertion of this reciprocal action between animal substances and the air ; and it is, we conceive, by the gradual operation of these agents that the animal carbon is released from its existing combination, and brought into a condition capable of uniting with the oxygen gas of the air. We therefore suppose a spontaneous change in the animal substance to precede the chemical union of its carbon with the surrounding oxygen ; and, consequently, this carbon may be considered rather to separate from the other ingredients, than to be removed by the attractive force of oxygen.

670. In support of this opinion, it may be observed, that, provided due degrees of moisture and heat be supplied, animal substances are prone to change, whether oxygen gas be, or be not, present ; and consequently, that gas is not essential to the separation of the elements of the compound. Undoubtedly, the presence of oxygen will greatly modify both the nature and extent of the changes which may take

* Rapports, &c. passim.

place ; but such modification does not prove that it is essential to every change. The present state of chemical science does not enable us to trace the modifications of change which the varied application of the agents, concerned in the decomposition of animal substances, would produce ; but many examples might be stated wherein such substances have undergone remarkable changes, in situations from which the atmosphere was entirely excluded.

671. So, likewise, animal substances undergo spontaneous changes which enable them to afford carbon, when they are confined in elastic fluids deprived of oxygen gas. Thus, M. Huber found, that bees, when confined in air, afforded carbon after all the oxygen was consumed (133.), and this substance then combined with the residual nitrogen, in the same manner as when it was afforded by vegetables ;—facts which afford direct proof that carbon escapes from animal substances, in consequence of the spontaneous changes which they undergo, and that the attractive force of oxygen is not necessary to effect its separation.

672. In support of the foregoing facts, the following experiments may, likewise, be stated. A piece of fresh mutton was placed in a jar containing about fifteen cubic inches of atmospheric air, inverted over mercury. The meat was supported on a small hoop that was fixed about half way up the jar, and, beneath the hoop, a glass cup, containing water of potassa, was placed. The air gradually diminished in volume, and, by the third day, the mercury had risen to about one-sixth of the height of the jar, at

which point it remained stationary for many days. The apparatus was then immersed in a trough of water, and the alkaline solution was withdrawn under water. A quantity of the residual air was then repeatedly washed in lime water, to remove all suspicion of the existence of carbonic acid in it. Five different portions of this air were next passed into separate glass tubes, so as to occupy about half their volume, and each tube was then filled with oxymuriatic acid gas, obtained from oxymuriate of potassa and muriatic acid. The tubes were then closely corked under water, and kept inverted in that fluid, and exposed to the light of day. In 24 hours, one of the tubes was uncorked under water, and the water immediately rose into it to a considerable height. A portion of the remaining gas was then passed into another tube, filled with pure lime water, but it produced in it no discolouration. After two or three transmissions, however, carbonic acid was rendered manifest by the whitish film which formed on the sides of the tube, and by the white threads of carbonate which, after a few minutes, fell down through the body of the liquid. The gases in the other tubes were examined in succession, and all afforded the same phenomena, when transmitted through lime water.

673. In farther confirmation of these facts we placed portions of meat in a jar containing equal parts of atmospheric air and hydrogen, and also in another jar of pure hydrogen gas. Both jars were inverted over water, and remained in their respective positions for ten days. The residual gases were then washed in lime water, till they ceased to produce

any effect in it. Portions of them were then mixed, with equal bulks of oxymuriatic gas, in glass tubes which were closely stopped. At the end of 24 hours, the mixed gases were opened under water, when a great diminution of volume took place; and the residual gas, after two or three transmissions through lime water, gave abundant evidence of the presence of carbonic acid gas. When the gaseous mixture, consisting of carburetted hydrogen and oxymuriatic acid gas, was exposed to the direct agency of the solar rays, a dense white cloud instantly appeared, which soon subsided. A diminution of the volume of gas was then immediately produced, and by the method already described, the presence of carbonic acid in the residual air was at once detected. This effect of the sun's rays in quickening the action of hydrogen and oxymuriatic gases was observed by M. M. Gay Lussac and Thenard*; and also by Mr Dalton†. These results, therefore, sufficiently prove, that animal as well as vegetable substances undergo such spontaneous changes as enable them to yield their carbon to the gases which may surround them; and they shew likewise that the carbon, which thus unites with nitrogen or hydrogen gas, may be again separated by the superior affinity of oxygen‡.

* Mem. d'Arcueil, tom. ii. p. 349.

† Chem. Phil. p. 300.

‡ We were led to this method of experiment by witnessing, in the late experiments of Mr Murray on the nature of oxymuriatic acid gas, the facility with which the carburetted gases and oxymuriatic gas act on each other at low temperatures in day light: and, by exposing the gaseous mixture to the direct rays of the

674. But whether the carbon, furnished by animal bodies, escape by virtue of a living action, or whether it be afforded under those spontaneous changes which all organised bodies experience, the actual combination of this substance with oxygen gas is purely chemical, and to this combination, effects, apparently similar, seem to succeed. Thus, by exposure to oxygen gas, black blood is rendered red, as well after its removal from the body, as during its transmission through the lungs; and so, likewise, recently cut flesh often appears nearly black, but, by exposure to the air, it assumes a florid hue. Whatever, therefore, be the mode in which the carbon is supplied to act on the air, the sensible effect produced in the animal fluids, both during life and after death, is precisely the same, and must, therefore, be equally referred to the operation of a chemical action.

675. But although the union between oxygen gas and carbon be, in all cases, purely chemical, and the immediate effects to which it gives rise, both during the continuance of living action and under spontaneous decomposition, be precisely the same; yet the facts, which have now been detailed, sufficiently establish a difference in the mode in which this carbon is supplied. For in the living body, the emission of

sun, the action was immediately accomplished. It may be proper to add, that when, in these experiments, the tubes have been closely stopped, the carbonic acid is not detected, till after the gas has been repeatedly transmitted through lime water, its appearance at first being prevented by the presence of muriatic acid gas.

carbon is always preceded by the motion of the blood, and must, therefore, be considered, like every other separation of matter from that fluid, as an animal function, carried on and maintained by an appropriate organic structure, and according to the laws which regulate the exercise of living action; while, in inanimate bodies, this carbon is yielded in the progress of those spontaneous changes which ultimately terminate in the dissolution of the animal compound. Between the termination of the living and the commencement of the dead process, a period, more or less long, appears to intervene; and this period will vary, in different cases, according to the natural constitution of the body, and the mode and degree in which the external agents are employed. If the body retain its susceptibility of action, these agents will restore its living functions; if this susceptibility be lost, they only serve to hasten its decomposition and decay. Thus nearly do the powers required to exhibit the phenomena of life and of dissolution approximate each other; and thus do two series of actions, in their effects and consequences so entirely distinct, proceed under the operation of the same external agents.

676. With respect to the actual place of union between the carbon and oxygen in living animals, we suppose it to happen at the surface of the respiratory organ, whatsoever be the form of the organ, or in whatever part of the body it be placed. By the motion of the animal fluids, which universally precedes this combination, the carbon is brought to this surface in a state fitted to combine, and the air,

at the same time, by the action of the respiratory organ, is presented to the same surface, where its oxygen combines with the exhaled carbon, and both pass off together in the form of carbonic acid gas. This chemical union, especially in the higher animals, is increased and promoted by the great extent of surface of the respiratory organ ; by the apparent attraction or adhesion subsisting between this surface and the air ; by whatever accelerates the motion of the blood, and increases the exhalation of carbonic matter ; and, lastly, by the constant supplies of fresh air furnished by the action of the respiratory organs.

677. In many animals, however, which belong to the inferior classes, and in the whole class of fishes, as well as in aquatic plants, the air is not presented to the respiratory organ in an elastic state, but through the medium of solution in water. We have seen that vegetable and animal substances possess the power of separating air from water ; and a similar separation we must suppose to be made by the bodies, and especially by the respiratory organs, of aquatic animals. By the exertion of this power, the oxygen gas, contained in water, is brought into contact with the carbon as it exhales from the animal system, and a chemical combination, with the usual phenomena, takes place. In aquatic animals, no cells or receptacles for containing air are provided, so that, as in some animals of the vermes class, the union takes place at the surface of the blood-vessels ; but in terrestrial animals, a cellular structure for receiving the air is interposed, at the surface of which the combi-

nation of the oxygen with the carbon is effected. Lastly, we may observe, that, in animals which live in water, the respiratory organ may be considered as external, and as being constantly moistened by the mass of surrounding fluid, while in those which breathe in air, this organ may be regarded as internal, and the portion of air that comes into contact with it as separated from the general mass, by which means the organ is protected from the effects of too rapid evaporation, and is always preserved in a moist and secreting state, a provision which the condition of aquatic animals did not render necessary.

678. To the mode in which we have supposed the carbon to be afforded by the lungs in respiration, it has been objected by Dr Bostock, that "it does not explain how the regular supply of this substance is, at each successive respiration, brought to the lungs in a state proper to be discharged *." We certainly admit, as this respectable writer states, that the carbon, in common with every other ingredient of the body, is derived primarily through the organs of digestion; but we do not suppose it to be excreted in its first transmission through the lungs. We regard carbon as a constituent part of the animal fluids, and consider it to be excreted in the lungs as long as the blood continues to move. Hence it follows that the excretory function in the lungs is only the *immediate* source of the carbon that is supplied to act upon the air; and that its *remoter* source must be sought in that function which, by supplying materials to the

* Edinburgh Medical Journal, NO. xiv. p. 165.

blood itself, enables it to support all the secretions, and to recruit and maintain the health and stability of the system. So long, therefore, as the blood continues to move, and the secretory functions continue to be performed, so long will carbon be supplied to act upon the air; but when the motion of the blood is suspended, or has finally ceased, then carbon is no longer furnished, nor is the air any longer changed.

ADDITIONS TO CHAP. VI.

OF THE PHENOMENA WHICH ARISE FROM THE CHANGES
INDUCED ON THE AIR BY THE LIVING FUNCTIONS
OF VEGETABLES AND ANIMALS.

679. **I**N this last chapter of our work, we endeavoured to prove that the extrication of the subtile matter of caloric constitutes the only observed phenomenon that attends the conversion of oxygen gas into carbonic acid in the processes of vegetation and respiration. In proof of this extrication of caloric, it was shewn (165.), that oxygen gas possesses a large portion of specific heat; that, by the conversion of this gas into carbonic acid, its heat (166.) is necessarily set free; and that vegetables and animals, which produce this particular change in the air, exhibit, in consequence, a higher degree of temperature. To the evidence adduced in support of these positions we have now but little to add, which we the less regret, as the facts already stated appear sufficient to establish the general inferences deduced from them.

680. In the vegetable kingdom we have remarked (167.), that germinating seeds, when accumulated together, exhibit a sensible rise of temperature; and in the process of malting, Dr Thomson has seen the radicles of barley, when kept without turning on the malt floor, shoot out half an inch in a single night, and the heat rise as high as 100° *.

681. Many facts have been related (169.) in support of the opinion that plants possess a power of producing heat. The experiments, however, of Mr Hunter, to shew that trees possess a temperature higher than that of the surrounding air, are by no means satisfactory. He bored holes in trees to the depth of eleven inches, and found, that, in the month of March, when the atmosphere was 57.5° , the thermometer in the tree stood at 55° only. In April, when the temperature of the air was 62° , that of the tree was only 56° ; but when, on succeeding days, the air fell to 47° , the tree then exceeded it in temperature by eight degrees; the day following, however, both the tree and the atmosphere were at 42° . In October, when vegetation began to decline, and the temperature of the air was at 51.5 , that of the tree was 55.5 ; and a few days afterwards, when the atmosphere was at 47° , the tree was from 5 to 6° warmer. In November, a similar difference in temperature was observed; but in December, both the tree and air were found, in one experiment, to be exactly 29° . In other instances, the temperature of the tree was sometimes higher and sometimes lower than

* Thomson's Syst. Chem. vol. v. p. 304. 4th edit.

that of the atmosphere, by a difference varying from one to four degrees *.

682. These varying and contradictory results oppose the belief that the tree possesses any natural power of steadily maintaining a temperature higher than that of the surrounding air, and lead to the supposition that its fluctuating condition, in this respect, proceeds entirely from accidental causes. Variations in atmospheric temperature must be supposed to influence that of the tree; but the rapidity with which changes occur in the former would not, in an equal degree, affect the latter. Thus, in summer, the temperature of the tree rose slowly, when that of the atmosphere was rapidly changing; and when the latter was as high as 57° , the heat of the tree was generally less. On the contrary, when the atmosphere fell below 57° , the tree was a few degrees warmer; but if the cold continued a few days, both the tree and the atmosphere came down to the same temperature, even in April when vegetation was actively going on. These facts seem to prove, that the tree possesses no internal power of producing heat; but that its temperature follows that of the surrounding air, subject to such variation as arises from a difference in its conducting power, and other accidental circumstances.

683. But whatever doubts may exist as to the natural temperature of trees, the facts already adduced (170.), concerning the heat exhibited by certain plants, during the process of fecundation, are too

* Phil. Trans. 1778, p. 46.

striking to be liable to any exception. In addition to the observations of M. Hubert on the heat of the *arum cordifolium*, we may now state others, made on another species of the same genus, by M. M. Lamarck and Senebier. M. Lamarck observed the flower of this plant, at the period of fecundation, to communicate the sensation of heat, and a similar observation was made by M. Senebier. The *maximum* of heat occurred about six hours after mid-day, when it exceeded that of the atmosphere by nearly 12° Fahren. During the development of this heat, the flower in part became black; and the rapid combination of oxygen with the carbon of the flower may, says M. Senebier, be suspected as the cause of this appearance: but he did not, by experiment, ascertain the fact *. The more accurate observations of M. Hubert (171.), have shewn that this great increase of heat is accomplished chiefly by the external surface of the spadix of the flower; that the presence of air is necessary to its production; and that, during its development, the pure part of the air is consumed; whence it may be inferred that this heat is immediately derived from the extrication of caloric by the consumption of oxygen gas.

684. We have already given examples (173. et seq.), which prove that worms, insects, fishes and reptiles, possess, in many instances; a temperature higher than that of the medium in which they live; and we have also maintained, that, as this small excess of heat is constantly passing off to the surrounding

* Physiol. Veg. tom. iii. p. 314. 317.

medium (178.), its permanence can be secured only by the constant exercise of some necessary animal function. But no function which the animal exerts, except that of respiration, can be conceived thus to afford a constant supply of heat ; and since, in the exercise of this function, the oxygen gas of the air is uniformly converted into carbonic acid, under which its latent caloric is necessarily set free, the liberation of this caloric by the perpetual decomposition of the air (180.) must be regarded as the natural and necessary means by which the animal temperature is sustained.

685. In the higher classes of animals, which possess a temperature greatly exceeding that of the surrounding medium, and which, under every vicissitude of heat and cold, preserve nearly an uniform degree of heat, we have referred the primary source of animal temperature to a similar extrication of caloric, arising from the decomposition of the air (189.), in the exercise of the respiratory function, as originally suggested by the illustrious Dr Black, and subsequently developed and confirmed by the elaborate researches of Dr Crawford. In support of this doctrine, it has been maintained, that the latent heat of the air is necessarily set free in the lungs (190.) during the exercise of the respiratory function ; that the blood, after this extrication of caloric, possesses an increased portion (191.) of specific heat ; and that the quantity of caloric thus actually afforded to that fluid may be considered sufficient (192. 3.) to account for the height and continuance of animal temperature.

686. To this doctrine of animal heat, we are aware that many objections have been made, and that some physiologists still seem disposed to question its validity. But whether we consider the natural constitution of the air, the actual changes which it suffers in the lungs, and the extrication of caloric that accompanies these changes; or whether we attend to the increase of specific heat acquired by the blood, and the universal relation which animal temperature bears to the extent and perfection of the respiratory organs; or whether, lastly, we contemplate the total insufficiency of every other known function or operation to afford that permanent supply of heat which the exigencies of the animal system require; we are equally conducted to the conclusion, that the change induced on the air by respiration is the true, sufficient, and only original source of animal heat. If, indeed, it be not to afford that subtile matter which it yields by its decomposition in the lungs, we know no essential purpose which the air can be considered to serve; for it has, we trust, been established (621.), by an induction as strict and ample as the nature of the case demands, that, in respiration, no part of the ponderable matter of the air enters into the blood, but that the conversion of its oxygenous portion into an equal bulk of carbonic acid gas constitutes the only necessary change which the air experiences during the exercise of this animal function.

SUCH is the result of his Farther Inquiries into the respiratory function of Plants and Animals which the Author submits to the Public. In all that he has yet written, he has chiefly confined himself to a consideration of the nature and extent of the changes which living animals and vegetables induce on the air ; and the amount of his researches may be comprised nearly in the simple statement, *That oxygen gas is uniformly converted into carbonic acid during the exercise of the respiratory function, and that, by this chemical change in the air, its latent or specific caloric is set free, and enters into the vegetable and animal systems.*

The facts which establish this particular change in the air, and the consequent entrance of its caloric into the system, may now, he conceives, be considered as fully and universally ascertained ; but the effects which this subtile matter afterwards produces, and the laws by which it is developed, have been less attentively regarded, and are, therefore, less perfectly understood. It was the Author's intention to have entered at once into a detailed investigation of these subjects ; but the unexpected length to which his present inquiries have extended, and a wish to settle definitively the preliminary questions now discussed, have again arrested his progress, and brought him to a temporary pause.

Should it, however, be thought, that, in the present work, he has succeeded in establishing the general facts which relate to the *changes induced on*

the air, and a desire be expressed that he proceed in his inquiries, he will yield a willing obedience to the command; and, in a subsequent volume, will endeavour to illustrate and explain the *reciprocal effects* which are produced in the vegetable and animal systems. Though duly sensible of the difficulties which he has to encounter, he yet hopes to be able to present a view of these subjects, adequate, as he thinks, to explain the phenomena, and free from the objections which lie against every explanation that has been hitherto proposed. In furtherance of this design, he now, therefore, ventures to announce his intention of attempting to trace *all the observed effects, which succeed to the exercise of the respiratory function in plants and animals, to the varied agency of that subtle or calorific matter, which is universally liberated, by the changes induced on the air, during the continuance of this living process.*

Ellis, Daniel

Farther inquiries into the changes induced on atmospheric air, by the
germination of seeds, the vegetation of plants, and the respiration of
animals

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